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DISCUSSIONS ON PAPERS PRESENTED TO CONGRESS.

No. 4—Disease Resistance.*

There was an excellent attendance in the Physics Laboratory, Armstrong College, Newcastle-upon-Tyne, at the morning session on Wednesday, September 5th, 1928, on the occasion of the discussion upon the paper by Dr. Crew on the subject of "Disease Resistance."

The Chair was occupied by Major-General Sir John Moore, who said he had much pleasure in presenting to the members Dr. Crew, who had been kind enough to write that very important and instructive paper. (Applause.)

As they knew, the discussion was to be opened by Mr. Miller, of Edinburgh, and he trusted they would have a good discussion on the subject.

Mr. W. C. Miller (Edinburgh): I feel that in attempting to open the discussion on Professor Crew's paper my capacities are totally inadequate to do justice either to the paper or the subject. I could have wished that someone older or more experienced had undertaken the task. I have read the paper and find that I can only agree with what I have read; that it does not admit of discussion in a truly critical manner; and that what is left for me is to pick from it one or two points, add to them one or two more ideas, and then attempt to contribute something to an examination of disease resistance from a somewhat different point of view.

The problem presented by a consideration of tuberculosis, as the essayist remarks, well illustrates the extraordinary complexity of the genetical analysis of disease, but granting that the differences in environment, previous contact with the disease or exposure to it, may be and in fact are responsible for much of the great variation in the animals or breeds which become attacked, there yet remains to be explained why certain individuals escape, even though exposed to equal chances of infection.

Probably many or most can recall individual instances among dairy cows where an individual has lived contiguously with one in an advanced stage of the disease, has had, one would imagine, every possible opportunity to become infected, and yet has escaped, and, moreover, continues to escape. Further, individual animals have been encountered during the course of experimental work which proved very resistant to artificial methods of infection. To cloak our lack of knowledge an explanation of this has been frequently offered by referring it to the effect of an unknown—or *x*—factor. An illustration of this kind is of little use by itself in the attempt to prove the existence of disease resistant factors in the constitution of an animal, but it must at least be looked upon as a link in the chain of evidence. One might add to it further by instancing the reputed comparative infrequence of tuberculosis in the Hereford as against the Shorthorn, and, in a wider sense, in domesticated buffaloes against domesticated cattle. At the same time it is somewhat difficult to be sure that variations of this nature among naturally-infected animals are not in reality rather the expression of varying conditions of environment than of natural resistance to the disease. Of more importance are the statements made by Kitasato that a definitely greater resistance to *controlled artificial* infection is exhibited by certain Japanese breeds of cattle, than is shewn by European breeds under the same conditions. I have been unable to trace further any experimental work of a genetical nature which goes to show that this greater resistance can be transmitted to the hybrid, or to indicate whether it is a dominant or recessive factor or group of factors, but if the reasoning is sound it would appear that careful hybridisation experiments along these lines might throw further light on the mechanisms of the inheritance of such disease resisting factors as do appear to exist in Japanese breeds. According to Hutyra and Marek, the cattle of the steppes of Southern Russia behave in a manner similar to the Japanese in that they also—comparatively free from tuberculosis in those territories where they are indigenous—show a very pronounced resistance against artificial infection in the laboratories.

On the converse, we have the authority of Dechambre of Alfort, and of Guérin, the collaborator of Calmette, for the statement that certain races of cattle possessed of a light-coloured coat and hide, contract tuberculosis more easily than those with dark coats, when they are placed together in the same infected byres—the most particularly susceptible in this respect being the light-roan Shorthorn (Dairy Shorthorn type) and the French Race des Landes; and that these breeds, on being crossed, transmit a portion of their susceptibility to their offspring. These statements are in accordance with those made by Landouzy and Guérin regarding the human race; that the blonde individual possessed of auburn, silky hair and pale, freckled skin of fine texture, shows a much greater degree of susceptibility to infection with tuberculosis than does the member of the Jewish race in the United States of America or the populous northern parts of Europe. Calmette, quoting Fishberg, gives the mortality from tuberculosis per 1,000 Jews in the United States of America as 37; while the total mortality from tuberculosis per 1,000 of the whole American population is 138. It would appear that the Jews are possessed of a certain degree of resistance against tuberculosis if these figures are to be relied upon.

The reference to the rôles which are possibly played by diatheses in the incidence of diseased conditions immediately focuses attention upon certain bony exostoses regarded as being of an hereditary nature, which commonly affect heavy draught horses—ringbones, spavins, splints and sidebones.

* Paper presented to the N.V.M.A. Congress, Newcastle-upon-Tyne, 5th September, 1928, by Professor F. A. E. Crew, M.D., D.Sc., Ph.D., F.R.S.E., Edinburgh.

With all due respect to the analysis of family history and pedigree, evidence has never yet been advanced which goes to prove that they are transmissible either directly or indirectly. The inclusion of some of these exostosed conditions as unsoundnesses, the possession of which unfits a stallion for breeding purposes, is open to some objection. Perhaps it is advisable, after making so heretical a statement, to offer some justification for it. The tendency is now more and more to look upon these conditions not as hereditary unsoundnesses due to the transmission to the progeny of a diathesis involving inherent weakness or imperfection of the bone-forming elements, but rather are they to be regarded as of the nature of exhibitions of something allied to the well-known rheumatoid arthritis of man. That is to say, given an animal with ancestry untainted so far as these bony enlargements are concerned, and rear it under conditions of an unhygienic and insanitary nature, and it will exhibit them ultimately. They may be intensified by bad shoeing, concussion, etc., but they are most probably never *caused* by them. On the other hand, and despite the bias which analysis of family pedigrees may show, a foal bred from a sire and dam each affected with ringbone, let us say, and even from grandparents similarly affected, will not itself develop ringbone, nor will its progeny, provided the environment be as nearly perfect as possible and the diet rational, especially during the formative ossifying period of life. To digress further for a moment, I may be permitted to make the observation that it is at least illogical to balance the minerals in the rations for our pigs and poultry, for calves and cattle, etc., and to supply them with fish-meal and cod-liver oil, while we pay little or no attention to these essentials in the rations of our young horses. I would hazard the opinion that were more attention paid to the food of a colt during the first three years of life, and were the general run of stables, horse-yards, and loose-boxes better constructed, especially so far as drainage, ventilation and natural lighting are concerned, we would hear very much less of ringbones and sidebones than we do. But this is a digression, permissible though it may be.

There appears to be an undoubted resistance against this group of bony diseases—which, for want of a better term, have been classed together by Continental writers as osteitisms—in certain of the pony breeds: in particular the Shetland, Iceland and Fells and Dales' breeds, and the Arab.

Whether this is an absolute resistance in the genetical sense, or whether it may be of the nature of a favourable diathesis, I am unaware. Perhaps others who have had opportunity to observe the incidence of these osteitisms under comparatively unfavourable conditions—such as Shetland ponies in coal mines—may be able to throw some further light on the question.

In passing, I would draw attention to the recorded fact that only in one solitary instance, by Stoll in a hare, has rachitis ever been observed in a wild animal living a natural life. Rachitis, also an osteitism, frequently affects the wild herbivores and carnivores kept in captivity in Zoological Gardens, but the truly feral animal escapes. There is little doubt that this is an example, not of a disease resistance in the natural wild animal, but rather of the induction of disease by unfavourable husbandry and environment.

Possibly one of the commonest conditions which may profitably be examined as an example of a more or less specific resistance is that of the immunity of the Hackney to sporadic lymphangitis. In this connection, probably the old writers upon "Water Farcy" were not so far wide of the mark when they advised that those mares of the heavy draught type which had had the water farcy should be subsequently mated only with light legged stallions. Until the pathologist has further unmasked the morbid changes occurring in lymphangitis, and until the exact rôle of diet and exercise and of external infecting organisms has been defined, it would seem that even yet we cannot improve upon that advice so far as the eradication of sporadic lymphangitis is concerned other than by avoiding breeding from the coarse-skinned, rough-coated, thick, gummy-legged, lymphatic individual of either sex.

On first thoughts the outbreak of swine erysipelas in East Anglia, and particularly in Cambridgeshire, during the summers of 1921 and 22, would seem to fit into the category of an enzoötic disease in which the resistance of the susceptible individuals rose from a low level to one that is now comparatively high. I would remind you that during the years mentioned the acute form of swine erysipelas raged through the Eastern Counties with a mortality of something between 70 per cent. and 80 per cent. of the young animals affected, but now the percentage death rate has fallen to about the normal level of not more than 10 per cent. to 15 per cent. (This is of course excluding the chronic verrucose endocarditis type of case, which practically always ends fatally). However, the facts are capable of explanation in other ways: for instance, firstly, the organism, being saprophytic, may have found external conditions during the years mentioned more suitable to its spread than normally; secondly, its virulence having become enhanced by frequent passage through different lots of pigs, it may have temporarily assumed characteristics more rapidly lethal than it normally possesses; and thirdly, the survivors of the acute outbreaks may constitute only a small percentage of the present local pig population—normally resistant pigs having been drafted in from surrounding unaffected areas. My reason for including this illustration is mostly to indicate the care which is necessary to eliminate other influencing factors before one can legitimately attribute to natural disease resistance effects which on casual examination appear to belong to it.

Montgomery records an interesting series of experiments undertaken during an outbreak of an exceedingly virulent or hyperacute type of swine fever in East Africa, which would appear to indicate that the Wart Hog (Phacochœrus sp.) and more especially the Uganda Bush Pig (Potamochœrus sp.) although able to harbour virulent infection in their blood, and able to act as "carriers," when infected experimentally, are immune, or at least very highly resistant to this East African virus of swine fever, which, between 1909 and 1912 gave a mortality of 98.9 per cent. among domesticated pigs on infected premises. It is not clear, however, whether or not these wild varieties are subject to the disease in the wild state, though after limited experimentation it was found that the wild pig remained free from infection even after living in close contact with infected domesticated pigs. It was only

converted into a carrier after artificial inoculation of virulent material.

This preliminary work would indicate that the wild pig, especially the Potamochœrus, is possessed of a natural hyperimmunity against swine fever of a form more virulent than is usually encountered in Great Britain.

It is an interesting fact that the single-toed Mulefoot breed of pigs which are reported to have been introduced into the U.S.A. by slave ships trading from the West Coast of Africa, were at one time regarded as being naturally immune to swine fever. I would remind you that the Mulefoot breed is unique amongst standardised breeds of pigs in that it possesses only a single well-developed claw and digit instead of a cloven hoof, and a small, low-set accessory digit—anatomical peculiarities which to a great extent have protected the breed from unrestrained admixture of other blood.

The widely advertised claims of swine fever immunity rapidly resulted in a frantic desire on the part of the American farmer to possess the breed. Good specimens were worth much money and the Mulefoot boomed. It soon became very evident that it possessed no greater immunity than that of other breeds, and its popularity fell as rapidly as it had previously risen. I have considered it worth while mentioning as an example of the inadvisability of making claims for naturally occurring immunity upon insecure foundations.

Glanders shows well-known variations in its symptomatology in different members of the Equidæ. Asses possess low powers of resistance and usually die from acute manifestations in from three to four weeks after infection. Recovery has not been recorded. Horses are subjected typically to chronic attacks which may extend for as long as six (Petrowsky) or seven (Dieckerhoff) years—the average duration being somewhere between nine months and two years, and according to numerous Continental observers a certain definite percentage of clinically-affected horses recovers completely. The disease in the mule is almost exactly intermediate. The early appearances are acute in nature, similar to but not identical with those in the ass, while the latter course of the disease resembles more closely the intermittent exacerbations and periods of subsidence which characterise the disease in the horse. Apart from the question of hybrid vigour, which must needs be remembered in this connection, the facts would suggest that the horse transmits to the mule a certain degree of resistance against infection by the *Bacillus mallei*. Unfortunately, from a genetical point of view, the matter must be left there, for, so far as I know, back crosses—horses ×mules, or mules ×asses—have not been tested in this connection.

Further, according to Schutz, Polish horses are possessed of a definite degree of breed resistance against glanders; they are infrequently infected by natural means, while those that are attacked suffer chronically and often recover; experimentally they are by no means so readily infected as German horses. Here again, however, I have failed to find any critical work undertaken to discover whether this resistance is a true genetical factor or is capable of explanation in other ways. As with tuberculosis, records show that individual animals in studs kept under uniform conditions and exposed to infection in the same manner as their co-mates quite often escape the disease, occasionally even escape it repeatedly. Some which have escaped naturally occurring attacks have been subjected to experimental infection but all attempts have failed to produce the disease, and these animals would appear to be veritably naturally immune.

Although the pathology of heartwater or "veldt sickness" in cattle, sheep and goats, in the Transvaal has not been exhaustively worked out, it is stated by Theiler that the Kaffir goat and the native Fat-tailed sheep are not susceptible, while the Persian sheep only develop a mild affection with slight symptoms, and usually recover. The non-susceptibility of the animals mentioned is not due to any tick-resisting powers of fleece or skin, for, according to Lounsberry and Robertson, ticks taken from them produced a fatal infection when placed upon susceptible sheep and goats. "Nairobi fever" of sheep and goats in Kenya affects native sheep with a mortality of 70 per cent. or more, while among infected Merinos about 70 per cent. recover. Academically, this might be of great interest if the figures were found to be at all constant under uniform conditions, but, as with heartwater, the pathology has not yet advanced sufficiently to allow of the elimination of outside variables.

The question may here naturally arise: Granting the existence of natural disease resistance in individuals, breeds or species of animals, how can it be used economically? Academically it may be of intriguing interest: Can it ever become of practical importance? He who would predict the future is a rash man, but I imagine the answer is "Yes."

When one considers that great complementary biological science—Botany—and when one remembers that botany also is a study of life; that certain fundamental principles were first noticed by Mendel in the sweet pea; that the laws of the mechanisms of inheritance and many others are common both to the animal and the plant, one is tempted to prophesy that just as the plant geneticist has bred strains of potatoes which will withstand black scab, strains of vines which ward off attack by phylloxera, of chestnut trees resistant to the bark fungus disease, and of blight resisting pear trees, to mention only a few, there will dawn a day when the animal geneticist will be able to do likewise for livestock.

Experiments have been in course for some years in Italy, the Argentine and in North America, whereby attempts are being made to induce the Zebu to transmit its capacity to resist tick and insect infestation and its immunity to anthrax and foot-and-mouth disease to its progeny when crossed with European breeds, and to improve the cattalo (Hereford ×Bison) so that it may be of more than curious interest. There are difficulties in the way, but they are not necessarily greater than others which have hitherto been overcome.

Finally, by merely mentioning such diseases as stringhalt, shivering, scrapie, joint-ill in foals and contagious abortion of cattle, in each of which there would seem to be some evidence of the existence of a diathesis, a hereditary dystrophy, or a transmitted influence (to speak loosely), perhaps I may pave the way for further discussion by those better qualified to speak on them than I am.

I must apologise for the discursiveness and even disjointedness of these remarks, but biology has hardly more than given birth to this her youngest child, and before we can predict a future for it or determine what manner of offspring it will become, it is perhaps permissible to rend off the fœtal wrappings even with a clumsy hand, and expose at least a small portion of its anatomy. (Applause.)

The CHAIRMAN (Sir John Moore): We have here a paper from so eminent a geneticist in Dr. Crew, and one so very ably opened in discussion by Mr. Miller, that I am sure large scope for discussion has been offered to our members, particularly from the practical side of disease resistance.

I would just like to say one or two words at the outset. The paper manifestly is one of cause and effect and, as I say, it offers to us great scope for the reconciliation of both cause and effect of the diseases that are presented to us in our daily practice. Of course, immunity or resistance to disease is very difficult to understand. It has many, many phases, and it recalls to my mind an incident that occurred in Dublin, many years ago, of two medical students who had been attending some scarlet fever patients. They were free from the disease until one played a violent game of football and the other some very exhausting game, and both these young men succumbed to the disease.

I would like to make a remark, to which perhaps Dr. Crew might make reference in his reply, about the normal temperatures of animals in relation to the existence of disease. It has often occurred to my mind that there is a great deal in the normal temperature variation—the horse's temperature varying from that of the cow and that of the dog, and also its being very high in birds.

In India, a country where anthrax prevails very severely and where it is extensively met with in cattle and sometimes in horses, I used to notice that in the disposal of carcases a common way was to take them out to the veldt and dump them down and allow them to be eaten by vultures, but one never heard of anthrax, or a disease simulating anthrax, in vultures. I think the reason for that was that they varied in their normal temperatures and that constituted a certain amount of resistance to disease.

I am very interested in the remarks made by Mr. Miller on glanders and in the relative occurrence of recoveries in different members of the Equidæ in cases of glanders. There are very few instances of recovery in glanders in horses, but I do remember seeing in the United States during the Boer War, when I was in command of a depôt of remounts there, a horse that had passed the mallein test, but which had extensive old ulcerations—indeed, a perforated septum—and I think that that was a recovered case of glanders. I was told, moreover, that the natural cure of glanders was not uncommon in the United States of America. Of the truth of this I am not prepared to say.

I have marked down here abortion in cows. A farmer told me yesterday that there is a prevailing idea in this district that if you put a goat in association with cattle you do not get abortion in cattle. I do not know whether there is anything in it.

On the subject of disease resistance and the theory of the reconciliation of cause and effect, and the whole subject of the causation of disease, I do not think we will progress to the desired end until there is good team work, and that not only amongst ourselves, but with the medical scientist and the plant pathologist. Sir Clifford Allbutt, who was our first President of the Comparative Section of the Royal Society of Medicine, showed in his Presidential Address that he was very keen on the association of the medical scientist, the veterinary scientist and the plant pathologist, and I certainly think he was right. (Applause.)

Dr. A. W. WHITEHOUSE (Principal, Glasgow Veterinary College): I am talking not as an anatomist, but in that capacity in which I have acted for a number of years—as a rather extensive breeder of cattle who was at the same time a veterinary surgeon and had some opportunity of observation. What I wish to suggest is that the disease of blackleg is probably peculiarly suited for experiment as to hereditary immunity. My own experience, and I think the experience of my fellow breeders, would indicate that a very considerable percentage of cattle of the susceptible age—probably more than 50 per cent.—had a very considerable resistance, *i.e.*, they were not at all likely to be infected with blackleg in the way (I think no one knows exactly) in the way the young cattle do get affected.

The incidence of it shows that you lose from 10—30 per cent. of the susceptible age unless you have vaccinated, and after that it stops. Knowing the way in which the preparing of blackleg vaccine is done, I think it possible that the commercial biological houses could help very much and could eventually determine whether there are animals which are practically insusceptible. With a minimum lethal dose you will find that there will be a very great variation. You will probably find that a number would be resistant even without vaccination, and from those you could get enough resistant animals to test out whether the resistance was to any extent hereditary.

Blackleg has the advantage over tuberculosis for such an investigation that the question of susceptibility can be determined in a very few days.

I was very much interested in Mr. Miller's point of view about the group of exostoses. I am very much in agreement with him, but my own views are still more heretical. But that is rather a red-herring across the path of this discussion, so I will not take it up.

Mr. BUCHANAN SMITH (Edinburgh): I must apologise for coming and talking here to-day, because I am not a veterinary surgeon, but Dr. Crew thought that some of the work we are doing for him in Edinburgh might be of interest to you, as it has a bearing on the subject. (Hear, hear.)

In the dim and distant ages there was a Royal Commission to determine hereditary defects in horses. It resulted in the Stallion Licensing Act of about ten years ago. The data of this Act are now available and as far as concerns Scotland we have been able to examine the pedigrees of horses which have been refused licences, and we are able to say, with a very considerable degree of certainty, that the majority of the points scheduled under the Act are definitely inherited. In regard to roaring, we can say of it that it is inherited as a simple Mendelian recessive. There you have a diathesis to some environmental cause, such as injury or overwork. In the same way you get a diathesis to a bacterial attack.

Dr. Crew's final analysis seems rather to leave the baby upon your doorstep. (Laughter.) He takes you so far and proves that these things are inherited, and he does

not take you very much further. He gives you another weapon for your armoury which might threaten to become a boomerang and do away with your profession altogether.

On the point of its possible application: it will be a long time before we can come to any real application of this theory of the inheritance of disease resistance. For instance, in foot-and-mouth disease, although there is a strong presumption in favour of resistance being inherited, it would be unwise to attempt to do anything about it in this country. There are, however, two possibilities which might come to the fore before so very long and be of practical use to the stock breeder. One would be swine fever. It has already been established in the United States of America at two experimental stations in Iowa and Illinois. Strains of pigs have been bred there which are definitely resistant to swine fever; they are not absolutely immune, but they are highly resistant. At the present moment these pigs are being graded up. It would be of extraordinary value to this country if one could, in a herd of pedigree pigs where an outbreak of swine fever has occurred, get any animals which happened to prove resistant.

Another possibility might be contagious abortion in cattle. I do not think that Dr. Crew mentioned in his paper some work at Wisconsin, where strains of guinea-pigs have been isolated which are highly resistant to the attacks of the *Bacillus abortus*, which in the average guinea-pig causes abortion. There are also to be found similar strains in cattle.

At the beginning of this year I was invited by the Colonial Office and the Government of Nigeria to visit Nigeria and West Africa with a view to enquiring into the possibility of transferring the high resistance of a small breed of cattle into the native-owned breed of Zebu cattle. The small cattle are bitten by the tsetse fly and their blood is full of trypanosomes; there is no doubt that they are all highly resistant to it, whereas the Zebu cattle are fairly susceptible to trypanosomiasis, especially if they are given any work to do. Trypanosomiasis does not yield to inoculation; therefore this looks as though it might be a problem where the geneticist might come to the aid of the stock breeder.

But, to my mind, the most important effect of this discussion, at any rate in this country, is the repercussion it should have upon our pedigree breeds. It is there that most of the trouble lies. We can trust our commercial stock to get rid of weaknesses for themselves. But the pedigree stock breeder, although he is a very small minority indeed in the country, values his stock too highly to send it to the butcher, with the result that an animal of good milking strain and performance has some weaknesses which can perhaps be fairly easily overcome. But the trouble accumulates and spreads throughout the breed.

In beef cattle I maintain that the worst trouble they have is sterility of varying sorts, but all sorts of weaknesses are cropping up, and as pedigree breeds go on more and more will they crop up. One of the latest I have heard about is a case in high milk producers of the Jersey breed where the cows apparently go down with some disorder that you really cannot call anything else than hay fever—asthmatic attacks which they only get in the summer time.

If the position is to be denied to the enemy as Dr. Crew says, that of our pedigree breeds which form the foundation of the others is the one that should be denied first.

The PRESIDENT (Mr. J. W. McIntosh): I should like, in the first place, to thank Dr. Crew for coming amongst us to give us this very excellent contribution. We look upon Dr. Crew as one of ourselves. (Hear, hear.) May I take this opportunity of congratulating him on the position to which he has been appointed quite recently and to say that we are looking forward to great results from his work in this highly important subject of Genetics. (Applause.)

I cannot, as an ordinary practitioner, attempt to criticise so highly technical a paper, but I do think that a great deal can be done to create a higher disease resistance in our animals. As to how far we may be able to succeed in establishing breeds which can resist disease, I do not know. It may be that in the near future disease will have become impossible.

Mr. Miller raised one point which rather surprised me—that is, with regard to certain exostoses, such as ringbone and sidebone. We were taught to believe that there is an hereditary predisposition transmitted through sire or dam to the offspring, and my own experience of thirty-five years supports that view. I can remember when mares were kept for breeding purposes which were unable to work on account of certain abnormalities such as ringbone, sidebones, etc., but were thought to be good enough for breeding purposes. In most cases it resulted in serious loss to the breeder, for as soon as the offspring was put to work nearly all of them developed ringbones or sidebones. The same thing is almost certain with diseases such as shivering, stringhalt and roaring, where the offspring of the animal affected with the disease invariably developed the peculiarities of the parent. I would like to hear Dr. Crew's remarks upon that point. My object in getting up is, however, to thank Dr. Crew for coming here and giving us this excellent contribution. (Applause.)

Mr. D. S. RABAGLIATI (Wakefield): I think it is very gratifying that such a great thinker as Dr. Crew should be with us to-day and should have taken so much trouble in preparing this particularly interesting paper. (Applause.) It is a paper which takes a great deal of thought to understand, and also it gives rise to very many questions of which we are all very ignorant.

If I am not giving away anything which is private property, I should like to say how glad we are that Mr. Miller has associated himself with Dr. Crew in the work of genetics. (Applause.) I have known Mr. Miller for many years and I am certain that no member of our profession would be more suitable for helping Dr. Crew in that work. We shall look forward to having something very valuable as a result of this co-operation. (Hear, hear.)

On page 110 Dr. Crew says: "According to this view, predisposition is merely proximity to the bacillus." Would Dr. Crew say, further, that immunity is due to distance from the bacillus? He also mentions the possibility of tuberculosis being due to a filter-passing virus of tubercle. I have not heard of this before—it may be my ignorance—but if it is so it will add to the complexity of dealing with tuberculosis. It seems to me that if one could find some factor which causes certain organisms usually present in the healthy animal to take on a pathogenic form, it would go a long way in helping us to produce immunity. But if we set ourselves to produce tubercle-free cattle, might we

not upset the balance and so produce cattle which might perhaps be less resistant to something else ?

Sir John Moore mentioned the question of goats and abortion. That is a very old idea and a very prevalent one. In Egypt a lot of the natives keep goats running amongst the cattle with the same idea. Mr. Powley mentioned to me just now what he has heard to be the genesis of this idea. He says that goats are very fond of eating ergot, of which they eat all they can get. There is thus less danger of the cattle eating it and so aborting.

I have often wondered whether the fact that goats being susceptible to the *Micrococcus melitensis* of Malta fever and the relationship of this organism to that of contagious abortion may have something to do with the idea.

Speaking of cattle resistant to tuberculosis, we found that of some thousands of Cyprus cattle imported into Egypt no case of tuberculosis existed. Some of these cattle were kept at the Egyptian Serum Institute along with native animals and yet none developed the disease, but in this case they were kept in open sheds.

During the War we found that the incidence of tuberculosis in camels varied greatly with the country from which they came. Tuberculosis is also practically unknown in Soudanese cattle, although the disease is rife in Egypt.

Glanders has been mentioned. The donkey is remarkable in always taking the disease in its acute form and yet we found in Egypt that the number of cases actually occurring in donkeys, even although there was ample means of infection, was very much less than in the horse, only an odd case occurring here and there while it used to be quite common in the horse.

Colonel G. K. WALKER (Abingdon): I wish to add my compliments to those of previous speakers to Dr. Crew, whom we are always very glad to see. I trust we may be able in the future to give him the title of "Disease Resister." (Laughter and applause.)

I shall preface my remarks by saying something about the resistance of Zebu cattle to disease. I think I may claim to have a little experience of Zebu cattle, if you regard India as the home of the breed. Previous speakers have spoken of Zebu cattle apart from India—*i.e.*, where they have been imported and are not found in their natural environment. Some confusion has arisen in regard to the resistance and immunity of Zebu cattle to disease. Dr. Crew has said that Zebu cattle in certain circumstances are immune to foot-and-mouth disease, also to piroplasmosis and to rinderpest. Well, Zebu cattle in India are susceptible to all these diseases, but there exists a very interesting and varying degree of resistance. The question is very complex, but, speaking generally, one is bound to think that environment must have something to do with the powers of resistance. Whether that environment is connected with housing, feeding, or climatic conditions one is unable to say, but we have always found in India that the cattle in the hills were more susceptible to many diseases than the cattle in the plains. For example, in fixing the dose of anti-rinderpest serum for the simultaneous method it has been found necessary to give 18 times the dose of serum to hill cattle as to plains cattle. That shows that the resistance of cattle in the hills is very much less than that of the cattle in the plains. The cattle in the plains also possess varying degrees of immunity. This acquired resistance does not seem to be due to enzootic causes entirely. These diseases are as rife in the hills as in the plains and may be regarded as more or less indigenous in India, yet, wide degrees of resistance are found.

At one time it was thought that tuberculosis was not found in cattle in India, but this is incorrect. Again the question of environment must be considered. The disease has been found in places where one expects to find tuberculosis in cattle, *i.e.*, where they are crowded together as in dairies. When they are kept in the open and under natural conditions the disease has not been reported. It has been found by experiment that Indian cattle have no special resistance to tuberculosis when inoculated with the virus.

One of the speakers said that Zebu cattle are very susceptible to trypanosomiasis. In India they may be said to be susceptible to both trypanosomiasis and piroplasmosis, but they are extremely resistant unless one disease is complicated with another or others. I have said sufficient to show you that the degree of resistance of Zebu cattle is extremely variable and the question of disease control is very complicated in consequence.

Dr. Crew at the beginning of his paper has referred to the eradication of disease, and later he has said that this must be a question of policy. This is a very serious matter for those who have to deal with cattle diseases from the administrative point of view. Dr. Crew has rightly said that it is high time that the parasitologist, protozoologist, serologist, immunologist, geneticist, pathologist and physiologist combined forces to attack the problem, but nevertheless it is clear that in a sense their interests conflict. A serologist, for instance, might be able to stamp out a disease temporarily by his methods, but should an outbreak occur, say ten years afterwards, the position would probably be worse then it was before, as any inherited disease resistance would have disappeared. Dr. Crew will probably tell us that that has nothing to do with policy, but it does affect men who have to deal with epizootic disease. The question actually came up for discussion in India. Whether we ought to try and prevent disease spreading by the injection of serum, or whether we should " let it rip " and allow the animals to develop their acquired resistance.

If the time comes when disease resistance in animal and man is established I suppose everybody will be pleased. As far as animals are concerned, excessive numbers can be controlled and, as far as human beings are concerned, it is said that populations can be controlled also. There is such a thing as birth control and with the increased dangers of transportation there does not appear to be much fear of over population at present. (Laughter and applause.)

Major T. G. HEATLEY (Woodbridge): I should like, first of all, to say that Dr. Crew's paper is far above my head. I am only a general practitioner, and it has struck me that during this Conference we have heard a great deal from the laboratory workers—and that has been an advantage to us general practitioners. (Hear, hear.)

I should like personally to thank Dr. Crew for the help he has given me, since I met him at our Congress at Aberdeen, in various points that have arisen. I should like an opinion from him as to the general observation

made amongst dairy cattle owners that probably the reason why the disease which is so prevalent now in dairy and other cattle—and that is sterility—is the fact that the milk producing functions of the genital organs are encouraged to the detriment of the primary breeding function. There is an impression amongst the dairy people that this is the reason why there is so much sterility in the country at the present time. I think myself that probably one of the reasons why we hear so much about sterility is that milk recording is responsible for a great deal of it. That statement cannot go by itself, because milk recording is very good. In the old days a man had a cow and she did not breed. The man did not know the true value of the cow, having no milk production record. Probably what she had was some disease of the genital organs which caused her to be sterile, and in those days if she would not breed she was fattened for the butcher. Nowadays, if a man has a cow giving 800—1,000 gallons of milk the great idea with him is to try and get a calf out of her, because the value of the calf would be great. If that cow has any disease she still remains in the byre, she is still served, and if there is any disease that is transmissible that cow is a source of danger to the herd. That is why I think milk recording may indirectly have been responsible for some of the sterility.

Someone raised a point about swine erysipelas. I think the responsible organism is always in the pig and all that is required for the disease to develop is the occurrence of some adverse condition. I believe that swine fever, under the present conditions, will never be eradicated. It has been brought to my notice very forcibly that carriers are prevalent. If you get an outbreak of swine fever you get no development for a time afterwards. After a while the premises are declared free and the pigs may be moved, but that does not do away with the carriers. I have advised one owner, if he wants still to keep breeding pigs, to castrate his boars to start with, to breed down the sows in pig, and clear everything right off the premises for a short time. Then he can start in fresh, in the hope that he can buy in young stock which are not carriers.

Concerning the goat, the idea is quite prevalent in my district, and I was once consulted as to whether a "Nanny" or "Billy" should be obtained. My advice was, that if the virtue lay in the smell, he ought to secure a "Billy."

Mr. HUGH BEGG (Hamilton): As an Inspector under the Diseases of Animals Acts I do work under the Insects and Pests Acts and would like to refer to one point. I have always been intrigued in regard to the complete immunity of certain potatoes such as "Golden Wonder" to the fungus that causes "wart disease." An ordinary examination of potatoes susceptible and of those immune to the attack of the *Synchitrium endobioticum* shows nothing in the colour, taste or section of the potatoes to account for the difference and yet an immune plant will grow its full crop in the midst of a badly diseased crop. Can Dr. Crew offer any explanation?

Referring to resistance to tuberculosis—some time ago we experienced a very surprising fall in a Grade A Tuberculin tested herd, thirty-two cows out of thirty-three reacting in one byre, although those in an adjacent but separate byre all passed as usual. All sorts of things have been said about this case by lay and professional men who will get their answer when the results of the second testing are published.

I draw Dr. Crew's attention to the fact that one aged cow in the infected byre stood the test in December when all its neighbours reacted, and when I retested the herd eight months later, using all the precautions imposed by the veterinary surgeons appointed by those who were critical of the first test, this old white cow gave an entirely negative result again.

Does Dr. Crew believe that there can be in our susceptible Ayrshire breed certain animals that have a real immunity to tuberculosis that they will not lose? If so, would he suggest that it would be possible to breed from such an animal an immune race of Ayrshire cows?

Professor W. A. POOL (Animal Diseases Research Association, Edinburgh): I associate myself with the other speakers concerning the able manner in which Dr. Crew has put the subject before us to-day. He is a pioneer and is opening up new territory which should be of great value to mankind. All will, I imagine, agree that there is every indication that resistance or susceptibility to disease may be genetical factors. We veterinarians are in contact with evidence of it every day and actually utilise our knowledge of it in the course of our work of eliminating losses due to disease.

In India we see this factor operating in the ravages wrought by rinderpest. Himalayan hill cattle free from admixture with "plains" breeds show very nearly a 100 per cent. mortality rate. Most plains breeds show a mortality of 20—30 per cent. We adjust the dosage of serum required for immunisation in accordance with this variation, giving the plains breeds as a rule about one-fifth of the dose we use for Himalayan cattle.

Dosage of serum required for immunisation does not, however, follow the natural mortality rate. European breeds which show a mortality rate at least near to that of Himalayan cattle, require more than four times the dose of serum per unit weight.

All will agree that if disease-resistant stocks can be bred more economically than individual cattle can be immunised, it is the better policy. The latter policy produces good results at short notice and will always be followed where the necessity arises. If the necessity can be avoided by dealing with the question at an earlier stage, in this case with the laying down of the foundations of the embryo, animal life will be the gainer.

This question of resistance to disease is of the greatest importance in connection with breeding policy in our Dominions and Colonies. Considerable importation of European breeds has been carried out in recent years. The crosses with native animals have been an enormous improvement in so far as milk production, conformation, etc., are concerned, but tropical disease has been a nightmare to those responsible for their maintenance.

A question yet to be settled is whether the better policy is to improve the naturally good native breeds with their high resistance to disease, or to improve them by mixing with the more delicate European breeds. The latter policy shows rapid results and is more pleasing to the individual concerned and provides kudos within his own lifetime. The former policy is slow and less spectacular.

My experience in India led me to think, so far as cattle are concerned, we would have done more good to the country by limiting ourselves to systematic improvement of the naturally resistant native breeds.

Scrapie in sheep may provide an example of an hereditary disease. It is almost confined to the Border-Leicesters, Cheviots and half-breds. Scotch Blackfaces are bred with Border-Leicesters to a very considerable extent in the severely infected areas of the country and in many instances must continually be mated with infected individuals. It is, however, quite difficult to find an actual instance of scrapie in a Blackface sheep.

Quite a large number of farmers are replacing Cheviots for Border-Leicesters to produce Greyface lambs instead of half-breds because they find that they get less scrapie in this way and that, though it would be more profitable to breed healthy half-breds, it pays them better to breed healthy Greyfaces than scrapie infected half-breds.

In conclusion, I would ask, is the Blackface immune and is the Greyface immmune ?

Mr. F. L. Gooch (Stamford): I wish to add my thanks to Dr. Crew for his paper and also for the fact that ever since the Aberdeen meeting he has helped veterinary surgeons in regard to the important matter of sterility. (Hear, hear.) In regard to blackleg, you will find certain farms where this disease is indigenous and where you never get the farm free from it; yet on the adjoining farm you never meet with a case. Certainly there must be a resistance there.

I think our Chairman's idea about the goats is an exploded one. I can assure him that if he sticks a goat among a herd of cattle pregnant for the first time he will soon have abortion. (Laughter.)

Mr. P. F. Dolan (Dublin): There are a few points in this paper to which I should like to make some reference, and though I am not in a position to criticise it I would like to become clearer in my views.

I do not think that Dr. Crew has sufficiently differentiated between immunity and this "special resistance"—at least, for those of us who have not carefully read the paper. (Laughter.) This might have the effect of allowing the public still more to rely upon resistance to a disease rather than to take proper preventative measures in preventing the disease. The public are, in many instances, ignorant that diseases like tuberculosis are due to organisms. Dr. Crew has specially mentioned tuberculosis and here again he has not distinguished between infection and the development of disease.

We know that a large number of animals become infected but the disease does not develop to any great extent in them. This is of importance in the disposal of reactors in the formation of tubercle-free herds.

Dr. Crew states, on page 110, that "malnutrition tends to increase the incidence of disease." This may be so, but we also know that animals fed on the best, and kept under the most suitable conditions, are very often extensively affected with disease; but, on the other hand, animals kept under scantier conditions are often free from disease.

On the same page Dr. Crew states that "within a race, breed, or strain it seems clear that there are individuals who, in virtue of their hereditary constitutions, are more susceptible to tuberculosis than are their fellows." Is this not a question of immunity and not of hereditary constitution ? And on this account should not the offspring of tuberculous parents be freer from tuberculosis, provided that they are prevented from becoming infected from the parents ? In building up tubercle-free herds infected animals are weeded out, and the remaining animals should have this special resistance, as said by Dr. Crew. But we know that these tubercle-free animals are very susceptible to infection and that if they are not prevented from coming in contact with infected animals they readily become infected and the infection spreads rapidly through the herd.

I think that more study and far more experimental work should be carried out in connection with this subject.

Major A. N. Foster (Uganda): Working, as I do, in Uganda, there is one point in connection with disease resistance which is a very well known one to us who have lived in the tropics, and that is the resistance of the black races to malaria, and also that blackwater fever, though it does occur in the negro, is extremely rare.

Trypanosomiasis has been mentioned and I think only one gentleman who has taken part in the discussion has mentioned the variety of trypanosome. I think that was Colonel Walker, who spoke about the trypanosome of Evans, which is pathogenic to horses and camels in India, and he pointed out that cattle are, if not resistant, at least very tolerant. We meet with a lot of trypanosomiasis in Uganda: it occurs amongst human beings and amongst cattle. It depends upon the variety of trypanosome, apparently, whether or not you are going to have mortality from it; also upon some factor yet unknown. For you can have cattle living in a trypanosome area with a very pathogenic variety of trypanosome and tsetse fly present, and they may go some years without serious trouble. Then there comes a time when disease breaks out with heavy mortality, and it is this unknown factor—as to what is operating in addition to the trypanosome—which is so important.

Getting down to quarter-evil and to anthrax, I think I am right in saying that both bacilli are facultative and sporulating parasites. What happens to them when living in the soil ? Is their virulence in any way exalted during that time ?

The incidence of rinderpest has been discussed. We cannot get a fixed virus, but have to use virus from cases we meet in the field for double inoculations, and I do think that there is something there which indicates the existence of disease resistance. There seems to be some unknown factor which exalts or attenuates the virus of rinderpest.

Mr. Arthur Gofton (Edinburgh): I did not intend to speak on this paper, but some of the remarks which have been made have tempted me to make one or two comments.

In Dr. Crew's paper, on page 119, there is a remark against which I put a cross when I read it. It is a remark with which I do not altogether agree. The remark is to the following effect: "The majority of tuberculin reacting cattle in which no lesions are found are young animals. The majority of cases with demonstrable lesions are older animals and the advanced and generalised cases are elderly cows. The physiological extravagance of heavy breeding and of milk production would seem to be responsible for the age differences. The decision concerning infectivity

is made in certain instances by the ability or otherwise of the individual to mobilise blood quickly at the site of attack."

Now, I do not know where Dr. Crew draws the line between young animals and those which he classes as the older animals, but I do wish to state that this remark of Dr. Crew's is not in keeping with my experience, for I do not think that there is any difference found as between younger and older animals in which we find a reaction and then subsequently fail to find a lesion.

As to the second part of the statement, with regard to the majority of cases with demonstrable lesions being older animals, I do not know whether he means the majority of cases of tuberculosis or the majority of reactors. If he means that the majority of cases of tuberculosis are found in the older animals, that again is contrary to my experience, unless Dr. Crew's expression "younger animals" refers to cattle under twelve or eighteen months of age. Concerning the third part: "The physiological extravagance of heavy breeding and of milk production would seem to be responsible for the age differences," again I cannot accept that statement, because it seems to me that the difference in the extent which tuberculosis prevails as between younger and older animals is not due to the effect of heavy breeding and milk production so much as to the intimacy of contact and the lack of fresh air and sunlight which the housing of the older cattle entails. As to the last statement that "The decision concerning infectivity is made in certain instances by the ability or otherwise of the individual to mobilise blood quickly at the site of attack," I do not know whether that is speculation or a statement of fact, but I want to refer to the case mentioned by Mr. Begg, because it seems to me that there is a more striking fact in connection with that case than Mr. Begg referred to. You have a herd of fifty cows (the numbers are purely illustrative) tested over a period of years with only an occasional reactor. After an interval of six months there is a sudden collapse of the herd: forty out of the fifty give a definite reaction to the test. Six months later the test is again applied to the cattle, which have been living under the same conditions. Nine out of the ten which failed to react are living in a separate byre, but are in contact out of doors. The same forty cows go down and the same ten stand up to the second test. It seems to me extraordinary that an acute infection which in the course of six months is capable of running through forty out of fifty cattle in the herd should stop at the fortieth cow. Why did it not extend to the other ten or some of them during the second six months? One is inclined to think that there must have been some unknown factor present to explain that.

In this connection, one might refer to the natural resistance of sheep to tuberculosis. It is generally stated that, in a large measure at least, the freedom of sheep from tuberculosis is due to the conditions under which they live. Is there not, in addition, some genetic factor which enables them to resist the infection? The number of cases of tuberculosis in sheep is extremely small and is not in proportion to the number of opportunities for infection. It is true that the great majority of sheep are not grazing in contact with cattle, but a large number of sheep do graze over the same ground as cattle. We know that the tubercle bacilli which are deposited on the ground by tuberculous cattle in their fæces are capable of maintaining their vitality for months. Sheep graze over the same ground and yet one does not meet with the proportion of cases of tuberculosis that one might expect. It seems to me that there is possibly a factor of genetic origin which would explain the resistance of sheep to that particular disease.

Mr. O. STINSON (Appleby): After hearing Mr. Begg and Mr. Gofton speak about tuberculosis, I thought I must say that I am of the decided opinion that tuberculosis in cattle is a disease which cannot co-exist with fresh air and sunlight. I believe it is a fact, in regard to human tuberculosis, that nurses never contract the disease, simply because they are nursing their tuberculous patients in fresh air.

With reference to diseases of the teat of the cow and hereditary predisposition, we can do a lot, as cattle practitioners, to eliminate chambered teat, bottled teat, etc., because practically all those conditions are spread by the bull.

I would suggest that this is an important point for cattle practitioners to remember.

THE REPLY.

Dr. CREW: I believe it was Colonel Walker who suggested that in time, with luck and with opportunity, I might earn for myself the title of "disease resister." I wish I could hope that synchronously I could earn for myself the title of "invitation resister," because whenever I am asked to come and open a discussion at this Congress it is truly impossible for me, in spite of all my preferences, to say no. (Hear, hear.)

It has been said by one whom I hope to reprove later—(laughter)—that the laboratory worker occupies the forum rather largely on these occasions and that these, and I am one, have little really to hand on to the man who is engaged in the practical affairs of breeding. It is unhappily true, and one has a natural hesitancy in talking on subjects like genetics, which are still so very academic. I chose this subject very deliberately, however, because in the first place, as is quite obvious already, I and everyone else know very little about it, and secondly because—and far more importantly because—it is entirely necessary that we should in collaboration discuss the facts and the theories, and the application of those facts and theories. I can tell you, from my knowledge of the workings of the minds of those who guide this State to its destiny, that a great deal of attention is now being paid to the possibilities of augmenting Imperial policies by the advocacy and practice of disease resistance in application. They are discussing already the possibilities of very extensive experimentation along the line of disease resistant stocks of animals and plants, in order to extend the conquest of man.

Therefore, although I was entirely innocent of anything really concrete on this subject, I thought it necessary that I should come to you, confess my ignorance and ask for your point of view.

In the first place it is for me dogmatically to express my opinion that the phenomena that can be interpreted in terms of genetical theory are so amazingly diverse and so intricate that there is nothing that can be exhibited by the organism, either a detail of structure or a detail of function, that, in my opinion, can prove to be of such a

kind as this hereditary mechanism cannot accommodate. If, as I can state dogmatically, all kinds and sorts of physical normalities and abnormalities can be, in their transmission from generation to generation, explained by rules of genetical fact and theory, then I know nothing relating to disease resistance or to immunity that is too difficult a thing to be tacked on to this theory.

I waded through the medical literature some time ago to find out what kind of, and how many, abnormal conditions have been recorded as being definitely inherited, and I produced, from a carefully-selected series of records, placing on one side all cases which contained an element of doubt, a mass of some 500 abnormal conditions—structural, physiological and psychological—that have been regarded by practical clinicians as being inherited. But I would warn you that I accept no responsibility for any statement that such and such a condition is inherited; I cannot—I do not know. These records, however, are the records of clinicians, and it is for me, as a geneticist, to take these results and try to accommodate them in terms of genetical theory; to put what I know against what they tell me, and find whether this mechanism can accommodate these new facts. If there is this mechanism, which can accommodate all the facts of experimental breeding with experimental animals under controlled conditions, and if this can also accommodate the facts that emerge from the records of the clinicians, it would be extravagant to postulate the existence of yet another kind of mechanism to provide for such phenomena as resistance and susceptibility. I maintain that it is reasonable for us to assume that this mechanism which can accommodate all the known instances of inheritance in experimental animals is, in its present form, sufficient to accommodate all the demands which are now, and are likely to be, made upon it in this matter of disease resistance and susceptibility. You will note that I am not responsible for the definitions. I am told that there is such a phenomenon as resistance, and such an alternative phenomenon as susceptibility, and I accept the definitions of other people. The first thing is to satisfy ourselves that disease resistance and susceptibility exist in fact, and I think we are entirely allowed to accept the actuality of these phenomena. There is any amount of evidence—most of it, I agree, circumstantial, but nevertheless very strong. I think we can agree that these phenomena do exist in fact. If this be so, then it is for us to consider whether or not breeding for resistance is a practical proposition. We cannot allow this matter to remain entirely one of academic interest. There was a time, no doubt, when in our academic centres, inscribed above the door there was to be found, "Knowledge for its own sake," but to-day circumstances have changed. Scientists are the favoured amongst the people; the scientist is the only one amongst the community to whom is given full opportunity for self expression. He can no longer accept State money merely to seek information, not caring what is done with it. I have to hope that what I find may be of use to the community. (Applause.)

We want to know, also, whether as practical people, trying to help those who are guiding the destinies of the State, and the community as a whole, it is sufficient for us to accept as an unexplained fact the existence of resistance, and whether it can be manipulated in breeding, or whether we ought to delve much deeper and to endeavour to reveal the elements of the defensive mechanisms of the animal body, because it is true so far that all breeding which has for its object the association of one type of a high resistance to some particular kind of deleterious condition with the economic qualities of imported races, has been entirely empirical. But in practice one does not need to wonder too much as to how it is that a resistant stock is resistant. Various views have been put forward, but it is unnecessary in these days to wait for a complete explanation of a defensive mechanism before we take advantage of it in our breeding practices.

Let me deal with the questions raised by individual speakers before I close with a peroration.

I should like very much to deal with those questions concerning tuberculosis in a general way, because to do so will answer quite a number of speakers and complement quite a number of remarks. I think it would be very helpful if we would contemplate the conception of the individual as a congeries of independently-inherited units. Moreover, the individual is a combination of derivatives of ecto-meso-and endo-derm. If you examine the mortality rates and disease incidence of any hospital for humanity, or any *post-mortem* room at a veterinary college, you will find that death was caused most commonly through breakdown in organ-systems of endo-dermal origin. It would seem reasonable to postulate that, in relation to the conditions in which the individual has to live, the organ-systems have in their evolutionary processes attained different levels of perfection. The ecto-dermal tissues have achieved a condition in which they are now entirely efficient in their functioning. The meso-derm in its evolutionary development has succeeded in keeping pace with the development of the social organism. In the case of human beings you will find that some 60 per cent. of all deaths are due to the breakdown in organ-systems of endo-dermal development—respiratory, circulatory and so on.

If it is reasonable to think of the rates of evolution of different organ-systems as being different, then it is easy, I suggest, to think of the efficiency of different organ-systems as being very different in different individuals and races. If this is so then it may well be that certain individuals, and certain groups of related individuals, will be characterised by an inherent tendency to re-attack of some organism which preferentially invades a particular system of the body.

I think I ought to chide you sorely because you will find that hardly a week passes without one of the medical journals citing instances of reputed inherited disease. This is not so in the case of the veterinary journals. I would suggest that it is important that the veterinarian should record, for the sake of fellows like myself, all the instances of seemingly inherited disease that come in his purview. Medical people are very much alive to the importance of recording familial incidence in disease. If you take respiratory diseases you can show with a little care and perhaps a little manipulation of your figures—(laughter)—and to your satisfaction that you can get individuals and families which are peculiar in that not only is there always an exhibition of inherited tendency to

breakdown of the respiratory system, but also that that breakdown shall occur at a certain age. You can show that with the mating of an individual from a strain with an inherited tendency to breakdown of the respiratory system in early childhood with a strain which is full of tuberculosis, the pooling of these developmental predispositions results in the production of a family giving 100 per cent. respiratory troubles, and you can show indubitably that these facts cannot be destroyed by any appeal to differences in experience—housing, clothing, work, and so forth. There is very strong presumptive evidence that there do exist in human families—and I would maintain also in cattle and sheep and so on—plentiful instances of strains which are remarkable for this tendency to develop a breakdown in some system on the slightest provocation. Speaker after speaker implied the necessity for a complete definition of environmental conditions when one began one's analysis. That is absolutely axiomatic. The geneticist should not speak in public concerning the hereditary nature of a condition until the specialists of animal nutrition have examined the circumstances and given a clean bill of health, because it is obviously true that the expression of any tendency to this or to that character will be entirely conditioned by the circumstances in which the individual is living. For example, most of what we know of the phenomena of heredity has emerged from the study of a creature in relation to which the student of heredity stands in the position of a god—the banana fly, *Drosophila melanogaster*. This is the only living thing of which we know sufficient to enable us to predict with absolute certainty what will happen if one does this or that under certain stated conditions. The reason is that one's experimentation with this fly is of such a kind as to give one absolute control of every environmental feature. Therefore there is nothing that you can compare with that fly, because you cannot have control of experimentation in the case of other forms of animal life to the same extent. Several speakers have suggested that it would be of great advantage if we investigated the hereditary possibility of genetic linkage between coat colour and the tendency to develop a disease, or the ability to withstand it. The question is of great importance. Linkage of this kind would enable us to select the unseen by choosing the seen. We know well that when one speaks of, say, a particular hereditary character in experimental material one is only referring to the most obvious effect of that developmental impulse which gives the character its name. For example, three is a factor in Drosophila for white eyes. One knows that white eyes is only the most obvious effect of the action of that developmental impulse; there are some twelve others. It may well be that there is a definite linkage between coat colour and an ability, or lack of it, to resist the attack of a disease-causing organism. But one cannot, at the present moment, state whether that linkage is real linkage or merely a chance association.

Mr. Miller and others have touched upon the question of the inheritance of certain exostoses. All I have got to say on this is that I am not responsible for the statement that these conditions are inheritable, that these are the observations of the clinician and that I accept the statement. Sir John Moore touched upon the relation of temperature to disease resistance and susceptibility. I know nothing of the relation of temperature to the activity of a pathological organism, save that I know that there are groups of organisms which can only flourish in a certain range of temperature. I am convinced, however, that it is a reasonable point of view, because it is established that cellular activity on the part of different types of cell within one's own body is conditioned by the temperature of the immediate surroundings. One has only to consider the condition of the undescended testis. It can be shown beyond all doubt that the failure on the part of the spermatocytes of the testis of the "rig" to become efficient spermatozoa is the result of the fact that the temperature within the abdominal cavity, or in the higher part of the inguinal canal, is higher than that in the scrotum. The nearer you get to the temperature of the scrotum the more complete is spermatogenesis in those cases. Arguing from such facts as these I was enabled to put forward the hypothesis that the scrotum is a temperature-regulating mechanism. If the temperature can govern the efficiency of the cells of the testis, surely it must have an equally important effect upon the living processes of uni-cellular and other organisms. My colleague, Mr. Buchanan Smith, referred to the work on contagious abortion and the possibility of resistance to the bacillus being inherited. A few weeks ago, a Japanese worker from Wisconsin, who was doing experimental work on rabbits, came to visit us. On the evidence he put before us it would seem beyond all doubt that certain individuals stand up to the *Bacillus abortus* with absolute completeness, whilst others do not, and that breeding experimentation yield a very typical segregation in the following generations of those who succumb, and of those who don't, if kept under the same conditions. This is a very pretty piece of work and, I think, very significant. It is incomplete at the moment, but one can say that the work promises to be a very valuable contribution to our knowledge. But is the rabbit vermin, or is it an animal? (Laughter.) Can you argue from the rabbit to the cow? Not always, and when you do one must be very cautious, and speak with any amount of reservation. But there are definite phenomena which are possessed in common by all animal forms, and I think it would be entirely unreasonable to argue that what applies to the rabbit does not and cannot apply to cattle; and this allows me to make this point, that it is entirely important that before we speak in public concerning the policy of the application of what fact or what theory we have got concerning disease resistance and immunity, we must see to it that experimentation with animals of economic importance has been undertaken. It is entirely important that critical experimentation with the actual animal concerned—in this case cattle—shall be undertaken. (Hear, hear.)

That, I think, is a part of my reply to Professor Whitehouse, who stated that blackleg is ideal material. We must urge that those concerned with policy shall see to it that we—not the geneticist, but the geneticist and the veterinary surgeon together—should have the opportunity of testing out in these animals of economic importance these theories that have emerged from the study of other forms. You will note that the evidence that Buchanan Smith put before you was derived from the examination of records, not experimentation, but the observation and analysis of statistical data. I am very impressed by the

evidence of statistics when that evidence confirms results of critical experimentation, but I am not prepared to advocate or defend the application of a point of view which is derived entirely from the contemplation of data that have been collected and treated statistically. It is, of course, impossible in a well-run hospital for anything to go wrong: you can prove it by an appeal to statistics. (Laughter.) But that does not help a fellow with a proctoscope through his peritoneum. (Renewed laughter.) I would not accept from statistics, as a fact, that ringbone, sidebone, stringhalt, etc., are inherited. I regard this evidence as strongly presumptive and sufficient to form the basis of a policy of stallion registration. Such evidence points the way to profitable experimentation.

Mr. McIntosh and others have been very kind in that they chose to congratulate me on my recent elevation. I am not sure that I am the gainer. (Laughter.) I have been told that I was making a profound mistake in not having a veterinary man on my staff. I agreed, and it has been indicated that we have now got such a man. (Hear, hear.) It is a matter of great gratification to me that circumstances have made this possible, and that it has been the development of the department that has made the coming of Mr. Miller so entirely necessary. (Applause.) This elevation that has come to me is really important only in that it gives to my subject an academic standing, and therefore it makes me more cautious in what I have got to say, because with dignity comes greater responsibility.

My old friend Mr. Rabagliati asks me what I mean by the proximity of the bacillus? I was arguing that since the disease-causing organism was so universally present this could not account for the fact that certain individuals succumbed to it whilst others did not.

I am not responsible for the suggestion that tuberculosis can be caused by a filter-passing organism; I am merely quoting, as I am forced to do, my authorities.

The goat has loomed very largely in this discussion. I have no knowledge of the matter at all, but if the goat is capable of keeping the *Bacillus abortus* away—I have a great respect for the *Bacillus abortus*. (Laughter.) I also dislike the company of the goat.

The points raised concerning the incidence of disease amongst Zebu cattle and amongst camels are of considerable importance, but in these cases, as in so many others, one has to be content with the observation that different species, or races, or breeds of the same family exhibit different resistances to disease-causing organisms. We will accept this, but that is not satisfactory: we must know whether this resistance or susceptibility is inherited, and inherited in a significantly orderly fashion. No really critical experimentation has been carried out in the larger animals which allows us to postulate that not only do these resistances and susceptibilities exist, but also that they are of a genetic nature.

I am sorry that I am not going to be here to-morrow, because I wanted to hear the discussion on education. It is a matter of some importance to me. I hold strongly that the science of genetics has developed to such an extent that it is important to-day that the veterinary student should become acquainted with its theories. (Applause.) The reason for that is this: I have reason to know that the education which the veterinary student gets is rather more appropriate to the practising geneticist than is that of the student of agriculture. (Applause.) I say dogmatically that I wish the opportunity and the responsibility of the control of animal disease through breeding to-day to be given to the veterinary surgeon, and I therefore want the veterinary student to be given an insight into the point of view of the geneticist. I think it is due to my science that in considering the education of the veterinary student you should give at least twelve hours to a man like myself—(laughter)—because in that time one could give a point of view which would have a profound effect upon the attitude of the man who is in a responsible position when he goes abroad. I want you to understand that I refute the suggestion that the general practitioner is unable to take his place side by side with the research worker in the pursuit of scientific knowledge. I gladly acknowledge that I am dependent upon the practitioner for the application of my science. I trust him absolutely—he being a professional scientist, whether he acknowledges it or not—to keep accurate records, and to supply me with them.

Major Heatley raised the important question of the significance of sterility in the increase of pedigree stock. That is a question of world-wide significance. The answer to Major Heatley is "I do not know" (the typical answer of the laboratory worker to the man of affairs). (Laughter.) The fact is that sterility is due to such a number of kinds of conditions that no one has ever been able to state that this particular instance of sterility is due to this cause or that—one simply does *not* know. (Laughter.) It is interesting that the sterility of dairy cattle has loomed so large, because the same problem is facing the breeder of highly-fecund fowls. The question is, in both cases, what is the physiological optimum of productivity? I personally am certain that one has got to look at this problem of sterility from that angle. What one knows with certainty is this: for the proper development of the progeny there must be harmony between two processes—the development of the uterus on the one hand, and the development of the fœtus on the other. It is possible, by the injecting of certain substances of unknown chemical constitution which are derived from the placenta, to prevent implantation of the fertilised ovum by causing a disharmonious—in rate—development of the uterus, so that by the time the ovum comes down the uterus is two or three hours ahead of the appropriate stage of development. I think that much sterility is due to this disharmony in the development of the uterus in relation to the development of the fertilised egg. If you get a disharmony you will get barrenness.

Mr. Begg helped us much by pointing out how absolutely akin was this problem in the case of plants. It is wonderful how, in a population of potatoes, you can see individuals expressing their constitutions in their relation to the development of such things as wart disease. If that can happen in the potato, I think something equivalent might happen in the pig. I think that ultimately the differences will be found to be in the chemistry of the cell itself.

He raised the important question of the age of the "white" cow and its resistance to disease. I can answer Mr. Gofton at the same time. I was quoting American

authorities when I was suggesting that there was a difference in the reaction to tuberculin by different-aged groups of cattle. There was an article which recently appeared in *The Journal of the American Veterinary Medical Association*. I must stand upon their statement. I should, however, be grateful if I may be introduced to Mr. Gofton's own records, because they apparently are not only contrary, but probably more numerous. If the statements concerning this aged white cow are to be accepted—and I accept them,—then I would hope quite reasonably to be able to find, by devious breeding means, a strain of individuals exhibiting the same character as does this aged white cow. In any case, the experiment simply yells for performance.

My colleague, Professor Pool, was very reticent and kind to me, because I have been encroaching largely upon his ground. I did it deliberately, because his Institute and mine have got to work in the closest harmony. (Hear, hear.) He raised the question of the policy of improving an indigenous stock by selection within the stock as opposed to hybridisation. I know practically nothing of the countries where hybridisation is being carried on very determinedly. I would hold, in general, however, from what I know of genetics, that it is a far sounder policy to improve a breed by selection within it than to improve by hybridisation, because in improving any stock, no matter how good, there is so much variation that I believe if one looked far enough one would find an ideal type from which one could make a much improved strain. It is entirely a matter of selection within a breed. I would have to be convinced that amongst the native indigenous breeds there were not specimens that were not infinitely superior to the average.

I have answered Mr. Gooch in a way. I cannot begin my analysis of the data supplied by the clinician until the pathologists have entirely finished their examination of the case. Probably absolute immunity is the rarest of phenomena and is the last stage in the scale of resistance.

Major Foster possesses an intimate knowledge of Uganda, and he raises an interesting question concerning the relative resistance to malaria of the different races. One is forced to enquire as to what has been the mechanism whereby the black races have come to possess their undoubted relative resistance to the disease. Has it been that, away back, certain people though attacked, survived, and thus developed an acquired resistance to the condition which they handed on to their descendants, so that when these were attacked they succumbed less readily, so that in time a resistant race developed ? Or was it that certain individuals, because of their genetic differences, did not contract the disease, and that they handed on this resistance ? One does not know.

He introduced another extraordinarily important point ; that is this question of the rhythm of the incidence of disease. I do not know whether any of you have read that wonderful book by Elton on Animal Ecology. He shows that this rhythm of disease is universal ; that you can predict, when you watch the fur markets of Canada, that a particular species will multiply exceedingly, and that when it reaches a certain density of population there will inevitably come an epidemic and down will go the population again. I would recommend any of you who are interested in this very spectacular phenomenon, the rhythm of epidemics, to get hold of Elton's book.

Mr. Gofton tripped me up on some really weak points in my paper. I have already explained that in the case of the age in relation to disease incidence I am quoting authorities. This problem of the hypothetical ten resisters touches upon the crux of the whole problem. What is the pedigree relation of the 10 and of the 40 ? How do they trace back ? Because if one only knew sufficient of their ancestry I am certain that one could explain that in the ancestry of the 10 there appears some factor which does not appear in the ancestry of the hypothetical 40.

Mr. Stinson again touched upon the point that diathesis can be obscured by optimum environmental conditions. It is absolutely important that those of us who are concerned with policies in breeding should always urge the breeder to watch not only the inheritance of the higher characters but also keep his eye upon those characters such as badly-formed teats, etc., which in themselves are quite insignificant but which, in relation to reproduction, are really all important.

Finally, what are we going to do about it ? This is really a very interesting point, and it permits one to adumbrate a very definite and fascinating philosophical point of view. It is a fact, in my experience, that one is right in assuming that in the pedigree herds now being bred there is a great mass of hidden characters of an undesirable kind, and that as breeding goes on there may appear, more and more commonly, those disadvantageous characters which will depreciate the value of stock. But does it matter ? Are we going to wipe out those which fall by the wayside ? Obviously it is not a question as to whether the individual shall live or not, but whether it shall reproduce or not. Let us assume for a moment that any man who lost his teeth at thirty should not be allowed to become a parent of the next generation. That would be absurd, because dentists can make dentures. Should we kill every individual who is a cretin ? The answer is "No," because the synthetic chemist can supply the deficiency which makes the individual abnormal. The time will come when every man will walk about with a veritable chemist's shop in his pocket to make good all his deficiencies. (Laughter.)* Does it matter ? I do not know. But obviously you can tackle this matter in one of two ways. You can wipe out all those individuals who have in their constitution an ingredient of disharmony, or you can supplement their deficiency by the products of organic chemistry. I am so deficient myself in so many ways that I prefer that the future should be left to the organic chemist. (Laughter and loud applause.)

The CHAIRMAN : I am sure you would wish me to express to Dr. Crew our very great appreciation, and the very great delight that we have experienced this morning in listening to his masterly reply. (Hear, hear.) The feeling that I have is one of extreme jealousy inasmuch as Dr. Crew, although it has been remarked that he is one of ourselves, is really not one of ourselves as a professional man, and personally I should have loved to have seen the degree of M.R.C.V.S. after Dr. Crew's name, then we should have had him for all time. (Applause.) I therefore hope

(Continued at foot of first column next page).

CLINICAL AND CASE REPORTS.

Observations on Twenty Consecutive Cases of the Bovine Disease known as Post-parturient Dyspepsia, Second Cleansing, Low Fever and, more recently, Acetonæmia.

By Oscar Stinson, M.R.C.V.S.,
Appleby.

[The following observations have an added interest from the publication, in the *Record* of September 22nd, of the Article by Mr. Richard Hudson, "Acetonæmia and its Association with the Retention of Uterine Fluids.—Ed.].

No. of Case.	Period since calving when attended first.	Presence of abnormal fluid in the uterus (quantity or character) or otherwise.	Presence or otherwise of smell of Acetone in milk.	Comment on case.	Result of Rothera's Test.
1	2 weeks	Normal	Yes	—	Positive
2	6 weeks	Abnormal	Yes	—	"
3	15 days	No uterine contents whatever	Yes	Heifer	"
4	6 weeks	Normal	Yes	—	"
5	19 days	Abnormal	No exaggerated new milk odour	—	"
7	3 weeks	Abnormal	Yes	Had aborted (contagious) 3 weeks previously (heifer)	"
8	18 days	Normal	Yes	—	"
9	13 days	Abnormal	Yes	Suffering from actinomyces at same time	"
10	17 days	Abnormal	No exaggerated new milk odour	—	"
11	5 weeks	No uterine contents whatever	Yes	—	"
12	22 days	No uterine contents whatever	Yes	—	"
13	17 days	Abnormal	Yes	Heifer	"
14	16 days	No uterine contents whatever	No exaggerated new milk odour	—	"
15	5 weeks	No uterine contents whatever	Yes	—	"
16	31 days	Normal	Yes	—	"
17	24 days	Abnormal	Yes	Was a serious case for embryotomy 24 days previous	"
18	13 days	Abnormal	Yes	—	"
19	A month	Normal	Yes	—	"
20	15 days	No uterine contents whatever	No exaggerated new milk odour	Heifer, at grass in June	"

Points of Interest.

Cases Nos. 5, 10, 14 and 20 had no odour of acetone in the milk, the presence of which has been looked upon as one of the characteristic symptoms of the disease. In view of this, and since these four cases proved positive to Rothera's test for acetone, the testing of milks for acetone in bovine disease within the first two months after calving is indicated when the diagnosis of a condition is doubtful. It will also be noticed that those same four cases had an *exaggerated new milk odour*. Observation requires, then, in these cases a keen sense of smell, as also is necessary in some cases of post-parturient dyspepsia, where the odour of acetone is faint. Most cases where there is an odour of acetone are observed by the average sense of smell on opening the byre door. In this connection, it is worthy of notice that there are some practitioners who cannot ever appreciate the peculiar smell of acetone. When testing milk by the sense of smell the writer always takes a sharp sniff while the nose is almost touching the milk. *The disease appears to have a distinct association with milk secretion.*

There is no disease in which a good milk yield can accompany such serious constitutional symptoms.

Turning the patient out to pasture, when the appetite is capricious, when she is not likely to eat enough to flush the milk yield, assists recovery, but the writer has a record of one case, in which, when the appetite returned to normal, she was turned out to grass, when she ate plentifully of this succulent milk forcing food; the milk yield increased, but the constitutional symptoms returned. She was immediately confined again to the byre when she

(*Continued from previous page.*)

that you will join with me in expressing the deepest and most sincere vote of thanks to Dr. Crew for being with us this morning and giving us such a stimulating intellectual treat.

I wish also, on your behalf, whole-heartedly to thank Mr. Miller for so ably opening the discussion to-day. I share with others who have made reference to it very great pleasure in the fact that there is an association between Mr. Miller and Dr. Crew, as I am convinced that it will redound to the credit of our profession. (Applause.)

received again a sustenance diet, and she soon improved. For a permanent cure I have found that the sustenance diet must not be increased until the patient's appetite has been normal for a week.

During the last two years two cases went down two hours after the administration of a purgative; in one case mag. sulph. was given, and in the other treacle. They both responded well to inflation of udder.

Inflation of the udder appears to be successful in a serious and prolonged case after all other treatments have been only palliative. It is used with good results when the subject is "down" and very excited. At other times, "inflation" has not appeared to be so successful.

Observations on Rothera's sodium-nitroprusside test. The writer has secured the opinion of a colour expert on a great many positive colour reaction rings of milk samples, and it appears that the colour of the rings is pale fuchsia, and not purple, as stated.

Out of the twenty cows, six had no uterine contents, five were considered to have normal uterine contents, and the remaining nine had abnormal uterine contents. It is interesting to note that case No. 7 had previously aborted; No. 17 had at the previous calving serious handling; No. 9 was actually suffering from another trouble at the same time; No. 20 calved outside in June, and was having practically no concentrates, she had no odour of acetone in the milk, but the exaggerated new milk odour, and proved positive to Rothera's test for acetone.

Most were very good milkers and none appeared to be recovering until the milk yield was seriously diminished. The writer has known three cases in the same building at the same time; the building was badly ventilated. Also the writer has known a farm where after putting the cows into a new, well-ventilated byre none were attacked, whereas previous to that the farmer had several cases every year. The feeding was similar in both buildings.

Experiment. Two cases were given a sustenance diet *plus* half a pint of linseed oil daily for three days, as a drench, with the result that the *patients gave more milk but clinically were much worse.* For the next two days a sustenance diet alone was given, with the result that both patients reverted to the condition before the administration of the oil. The following three days 1lb. treacle was given night and morning as well as a sustenance diet, with the result that the subject gave no more milk, but both were clinically decidedly better.

An Atypical Case of Tetanus.

By J. M. Buchanan, M.R.C.V.S., Stowmarket.

On the 7th August I was called to examine a four-year-old pedigree Suffolk gelding for soundness. I passed him as sound and thought no more of the matter. On the 11th I received a telephone message to come and see the same animal, as his owner could not understand a sudden onset of "nerves."

The animal was in a box, and on the horseman attempting to halter him the typical flashing of the membrana nectitans was seen. He seemed afraid to move, carried his hind quarters "woodenly," and stumbled at every step. There was no locking of the jaws, temperature normal, pulse small and hard and 40 per minute. No trace of a wound could be found and no history of even a scratch could be traced. Tetanus was diagnosed and the box darkened, sloppy food and the usual routine lockjaw treatment ordered. Twenty Behring units Anti-tetanic serum were injected subcutaneously and gradually increased by twenty every third day till eighty had been given. By then the animal moved better, was less nervous and eventually made an uneventful recovery. The only legacy of the trouble was an articular thoroughpin and bog spavin due to the prolonged standing. These conditions will, I expect, disappear as the joints strengthen.

The interest of the case to me at least was in the disease being wholly confined to the hind quarters—no loss of appetite or "lockjaw" appeared. The query arises, Was there a wound on the hind legs minute enough to be missed on examination and yet be the portal of entry of the bacillus?

Opening of the Session at Camden Town.

The new session of the Royal Veterinary College was inaugurated by an opening ceremony, on Tuesday of last week, which was presided over by Sir Archibald Weigall, the Treasurer of the Institution. In the presence of a large number of students, friends and old graduates, Sir Archibald opened the proceedings. He said that the College was on the verge of a new era. No one knew what line the new era might take, but there was a possibility that the Government might take on its shoulders its proper quota of responsibility.

Veterinary Science had been sadly neglected by the Government of this country, and in saying this he was not implicating any one party, but the Government as a State institution. He introduced to the meeting Sir Merrik Burrell, Chairman of the Executive Committee of the College.

Sir Merrik stressed the difficulties of the present situation, but stated that things were not without hope. He said that the present difficulties were not of the Governors' making, and that after the manner of a clergyman, he would plead for co-operation between Governors, staff and students in the College's hour of need.

From the purely financial point of view the College was bankrupt, but education must go on and the Government must help them to find a way out.

Sir Merrik was followed by Sir John Moore, who, speaking in a Biblical strain, pleaded for all to be faithful to their *Alma Mater.*

Professor J. T. Share-Jones, President of the Royal College of Veterinary Surgeons, in a speech not without humour, was strong in his denunciation of the Governments which had successively neglected the College.

Principal Hobday said they were looking for a minimum of £250,000—a sum which was easy of attainment if every sportsman and woman in the country would give only £1.

The session gave great promise: the entry of new students was the largest for many years.

The meeting closed with the usual votes of thanks and then visitors had the opportunity of seeing the work done and the present dilapidated state of the institution.

Royal Sanitary Institute's Congress, 1929.

The Council of the Royal Sanitary Institute has accepted the invitation of the Sheffield City Council to hold its 40th Congress and Health Exhibition at Sheffield from July 13th to 20th, 1929.

A Public Meeting to inaugurate the arrangements will be held in the Town Hall, Sheffield, on Friday, October 19th, at 3 p.m., under the chairmanship of the Right Hon. The Lord Mayor of Sheffield.

REVIEW.

[An Elementary Text Book of General Microbology. By WARD GILTNER, Professor of Bacteriology and Hygiene, Michigan State College. (Published by J. and A. Churchill, London, 1928. Price 15s.)].

This work covers a large field, and no doubt it would impress a student with the great range of the activities of the microscopic and ultra-microscopic forms of life in connection with disease and industry.

The term "Microbology" is introduced to include "the biology of the small forms of life."

A perusal of the 444 pages of the book leaves one wondering to what extent Professor Giltner is likely to attain his object, which is to present the subject in such a form that it will be "as understandable as possible to the college sophomore, regardless of his previous curriculum," and that it "will imbue him with a desire for further and deeper study of the subject of general microbology."

There is reason to doubt whether a student "regardless of his previous curriculum" could understand much of the text, which is too incomplete and inexact to be of much service to a more advanced student.

There are several plates, drawings and tables. Some of the drawings (and in this respect the book under review is not an exception) are a cobweb of lines reminiscent of drawings by some well-known artists whose efforts appear in that section of the lay press which is associated with humour.

In some of the tables, and particularly the one on page 44 which deals with the Schizomycetes, the printing is so small and indistinct as to be practically illegible.

A plate appears on page 343 purporting to show anthrax bacilli. Encapsulation of the organisms will be detected by one acquainted with the subject, but no reference is made to the capsule of the anthrax bacillus in the text, and the reference to the staining characteristics of the bacillus omits mention of the phenomenon observed in the methylene blue reaction.

On page 348, it is indicated that *Clostridium chauvei* causes black-leg in young cattle, and that "it is perhaps naturally pathogenic for sheep." Why the word "perhaps" is introduced in this connection is not clear.

In referring to *Mycobacterium paratuberculosis*—the causal organism of Johne's disease,—the author says this is also the cause of scrapie in English sheep, a statement for which there appears to be no authority.

In connection with avian tuberculin, one reads "mammals do not react to avian tuberculin unless they are affected with avian tuberculosis." In view of the use made of this product in the past for the diagnosis of Johne's disease, and what is taught in relation to it, this statement would appear to require some modification.

Professor Giltner remarks that glanders "is incurable and invariably fatal in man." It may be that the disease is usually fatal in man, but recovery in man is not unknown, *vide Jnl. Comp. Path. and Therapeutics*, Vol. XXVI, which contains the record of the recovery from glanders of a member of the veterinary profession.

In a work which deals with so many aspects of the subject, it would be unfair to expect completeness, but defective editing and incorrect information should be avoided. American spelling is no doubt legitimate, but errors in grammar appear to indicate hurry or carelessness. On page 159 one finds the following:—"consequently, the ways whereby it gains access to water supplies is no longer a mystery," and on page 215 "dairyman" reads "diaryman."

There are perhaps few books which provide no useful return for the time expended in reading them. In leisure moments a sophomore might with advantage read this book and enjoy a brief sojourn in what the author terms the "microbial wonderland." It is questionable if the student would be spending his time well if he attempted to "thoroughly master this elementary text." It would probably be better if he were to regard it as a rather interesting survey of the subject, and let it go at that, but fifteen shillings is too much to pay for a survey the general indications of which should be obtainable for less money, and with the expenditure of much less time.

S. J. M.

ABSTRACTS.

[Cases of Avian Diphtheria caused by the Klebs-Loffler Bacillus. POPOV., I. V., *bestrik Sovremennoi Veterinarii*, 3(52), Feb., 1928.]

The investigations were carried out at the Soviet Veterinary School in the Crimea. The affected birds showed symptoms varying from simple coryza to marked diphtheroses involving the mouth cavity and other parts.

Bacteriological examination of several cases revealed a mixed flora, including an organism subsequently identified as the Klebs-Löffler bacillus by the following characters: granules with Neisser's strain: Gram-positive, acid in dextrose and mannite media: hæmolysis in Martin's broth containing washed erythrocytes: virulent for guinea-pigs, pigeons and hens. Anti-Klebs-Löffler serum protected against the cultures from hens, whereas the controls died.

Popov emphasises that he was not dealing with pseudo-diphtheria bacilli, which are shorter, less granular, and show a more abundant and moister growth with less wavey edges on Löffler's serum. Further distinguishing points are the virulence of the Klebs-Löffler bacillus for guinea-pigs and its ability to break down sugars.

The author concludes that:—

1. The Klebs-Löffler bacillus isolated can produce a primary diphtheresis on the damaged mucous membrane of hens, pigeons and guinea-pigs.
2. The cause of avian diphtheria and the so-called colds in fowls is the same.
3. The only lesions present in avian diphtheria may be congestion of the mucous membranes and inflammation of the internal organs. Diphtheresis is not an essential termination to Klebs-Löffler infection.
4. As in humans, Klebs-Löffler infection is localised and does not become septicæmic.
5. Humans may harbour the Klebs-Löffler bacillus for two years. The same question requires investigation regarding birds.
6. Hens and pigeons behave similarly in relation to infection with the Klebs-Löffler bacillus.

7. Our conception of diphtheria in animals should be based on clinical and bacteriological findings rather than on anatomical appearances.

8. Scarification of the skin with the Klebs-Löffler bacillus does not produce chicken pox nodules.

J. P. R.

[Nieschulz (O.) Ueber die Möglichkeit biologischer Surrabekämpfung. Zoologische Beiträge zum Surra-problem Nr. XIII. [**The Possibility of the Biological Control of Surra. Zoological Contribution to the Surra Problem. No. XIII.**]— *Abhandl. a. d. Gebiet d. Auslands-kunde. Hamburg Univ.* Vol. 26. (Series D. Med. and Vet. Vol. 2.) [Festschrift Nocht.] pp. 380-385. [From a reprint.]—Ex. *Trop. Vet. Bull.*]

The introduction of Bayer 205 has rendered possible a reduction in the outbreaks of surra among horses, but the eradication of the disease is still a problem on account of the presence of infected cattle and buffaloes which are clinically healthy.

Nieschulz, during a tour of Java and Sumatra, considered the possibility of tackling the problem on biological lines.

In the first part of this brief paper the author summarises our knowledge regarding the transmitting agents of surra, and concludes that the place of prime importance must be given to the Tabanidæ, although it must be admitted that other insects play some part.

An attack upon the problem from a biological basis must therefore be aimed at destruction of Tabanidæ.

In the second part of the paper the possibilities of achieving this in our present state of knowledge are touched upon. The Tabanidæ are susceptible to attack in various stages of development, but it is admitted at once that attack upon the adult fly is out of the question altogether. So far, no one has been able to devise any practical and economical means of protecting animals against the bites of flies. In Java at least it is not practicable to paraffin the drinking places, nor can fly lime be spread on trees with any certainty of effect.

The natural enemies of the Tabanidæ play a very small part in their destruction.

It is not practicable to collect the egg packets. Infection of eggs with Phanurus plays some part in their destruction, but in nature there appears to be a balance struck between the eggs and the parasite.

Generally speaking, so far as our present knowledge goes there is no biological method of attacking the problem of surra.

The Preparation of Vaccine Lymphs.

The Commission on small-pox and vaccination of the Health Organisation of the League of Nations has issued a memorandum reporting the findings of its sub-committee on the preparation and preservation of vaccine lymphs.

The Commission, while not dealing with the technique of preparing lymph ready for issue, emphasises the vital need for veterinary control of the animals used, such control involving examination of the living animal before inoculation and the slaughter and *post-mortem* examination of the animal as soon as the vaccine has been obtained. The lymph should be used only when the results of these examinations are unexceptionable, and, in particular, when there are no signs of tuberculous lesions.

DIVISIONAL REPORT.

North of Scotland Division.*

President's Address at Aberdeen.

The half-yearly meeting of this Division was held in Marischal College, Aberdeen, on Saturday, August 25th, 1928, with Mr. R. E. Drennan (President) in the chair.

There were also present: Messrs. W. Anderson, J. Anderson, D. Cumming, D. Clerk, G. Howie, J. Lyon, H. MacVean, W. J. Rice, J. B. Russell, A. Sievwright, J. Burgess, and J. McAllan (Hon. Secretary).

The minutes of last meeting, as they appeared in the *Veterinary Record*, were taken as read and were approved.

Apologies for absence were received from Messrs. Brown (Aberdeen), H. W. Robson and W. J. B. Robson (Laurencekirk), D. Crabb (New Aberdour), and G. W. Begg (Inverness).

Correspondence. The Secretary read a letter, dated 25th February, from the National Veterinary Medical Association, with regard to contributions to the *Veterinary Record*. The purport of the letter was emphasised by the President, the Secretary and other members, and it was strongly urged by Mr. Drennan that members should do everything in their power to collect material for contributions to the *Record*, and so help in keeping up the very high standard which that journal has attained.

Letters dated 3rd April, 1928, and 21st July, 1928, from Mr. W. Gardner, Maybole, President of the Scottish Branch of the National Veterinary Medical Association, were read. These letters dealt with the financial position of the Branch, and asked the North of Scotland Division to consider the possibility of sending a contribution to carry on the excellent work which the Branch has been doing. After discussion it was proposed by Mr. James Anderson, and seconded by Mr. G. Howie, that the Division send an annual subscription of £2 2s. to the Branch.

New Member. Mr. Lyon (Banff) proposed, and Mr. Drennan (Aberdeen) seconded, that Mr. W. A. R. Ogilvie (Turriff) be elected a member of the Division, and this was unanimously agreed to. The President welcomed Mr. Ogilvie.

The question of the programme for the next meeting was left in the hands of the Secretary to decide. The feeling of the meeting was that some one outside the Division might be asked to give a paper on some branch of veterinary work.

Mr. James Anderson (Keith) in a few well-chosen words impressed the members with the excellent work being carried out by the Victoria Veterinary Benevolent Association, and stressed the great need of contributions to the funds of the Association. Whilst emphasising that every member of the profession should become a contributing member, Mr. Anderson also suggested that a collecting box might be procured and made use of at every meeting of the Division. The Secretary agreed to provide such a box at the next meeting.

Presidential Address.

Mr. Drennan then read his Presidential Address, which was highly appreciated by the members. The address

* Received on September 14th, 1928.

was as follows:—I have to thank you all for the honour you have conferred on me by appointing me President of this most enterprising Division. Had I attempted to visualise the President of a Veterinary Medical Association in my pre-veterinary days I think my conception of such a person would probably have been very much like the description given by one of our southern friends some years ago, as that of a dignified old person with head devoid of hair, his face disfigured by spectacles, and possibly the possessor or one or more flat feet! To-day, I find myself a President without having qualified for this honourable position.

It will be a very difficult matter for me to occupy the chair with the same dignity and grace as my predecessor, but I hope you will accord to me the same loyalty, and accept my assurance that the duties which may devolve upon me in connection with the office will be carried out to the best of my ability.

I have been long enough amongst you, I think, for you to have no misunderstanding as to my abilities, and I place on your shoulders all responsibility for any mistakes I may make. I can only say that if any difficulties should arise in connection with the office I will rely entirely on our worthy and honourable Secretary to keep me on the straight path.

It is the usual custom for the newly-appointed President to endeavour to review some of the events of the last year, but so much has been said and written that I feel I cannot add any new material. One thing we must all keep in view is the value to the profession of the scientific study of diseases of animals. Even at the present time it is only here and there that the practitioner receives encouragement to ask for laboratory collaboration in the investigation of some every-day problem.

It is true that the investigation of certain diseases, such as grass disease in horses and also canine distemper, is now being undertaken seriously, but this would not have yet been the case had not private enterprise forced the issue by supplying the necessary financial support, thus making the road possible to be travelled for a certain period only. If the solution of these problems can be found, the cost, whatever it may amount to, will not have been too great.

Many other problems equally important and worthy of conquest await those who have sufficient understanding to realise the important place even the canine patient has now taken in the veterinary practitioner's daily round. The advances made in surgery by the application of antiseptic principles and up-to-date skilful methods have restored thousands of animal pets to their grateful owners; but the solutions of the problems of medical diseases have not kept pace as they should have done, and there are still a very large number which baffle the veterinary surgeon in practice and even the research worker. As examples, canine and feline distemper, follicular mange, Stuttgart's disease, paralysis, rheumatism, certain forms of meningitis, chorea, and the so-called hysteria of the dog are only a few of the numerous medical disease problems which need elucidation. With so much naturally-affected material at our door it is astonishing that the laboratory workers seem so loth to step into the gap and centralise their efforts upon these abstruse problems until a prophylactic or a specific cure has been discovered. Success in this direction would not only earn the gratitude of the owners of valuable animals, but also that of the practising clinician, who has neither the time nor the opportunity for researches of this kind. There is a tremendous opening in Great Britain for whole-time research workers in animal disease problems, and their solution may not only help the animals for whose benefit the research is undertaken, but may in some instances be applied by the comparative thinker to the benefit of man.

Foot-and-mouth disease has unfortunately been fairly prevalent in certain areas of the United Kingdom, and despite the numerous investigations of the Ministry of Agriculture and others, we seem to be as far off as ever as to causation. I have a strong feeling that a very important factor (other than the actual causal organism) is the method of spread, and I feel the Ministry should spare neither money nor men in investigating each outbreak and tracing the source of infection.

The working of the Tuberculosis Order has certainly benefited the profession to a very great extent, and a great responsibility rests on all veterinary inspectors, whole-time or otherwise, who have to administer it, more especially in those areas where the Local Authority relies on its veterinary officer for an agreed market value of the animal. The Order, as far as I am concerned, will never stamp out tuberculosis in cattle. It will require drastic alteration to do so, but I am sure that it will greatly assist in the suppression of human tuberculosis. It is surprising the number of cases of tuberculous udders even in the early stages which are now reported by the dairymen and farmers since the Order came into force, but had it not been for the penalty attached, the milk from many of these animals would for a considerable period have been passed on for human consumption.

The question of the possible immunisation or vaccination of cattle against tuberculosis is of supreme importance, and as practically all cattle are born free of any tuberculosis (infection taking place after birth) the problem ought to admit of solution.

Experiments have been extensively carried out by immunisation methods, mostly, I should say, by the *French scientists*, but their results so far have been subject to controversy. The protection of the community by some safe scientific method of active immunisation even to a limited degree would be a great step towards the eradication of this apparently preventible scourge.

The problem of human immunity is much more difficult on account of the fact that children are susceptible to both human and bovine infection. One has only to consider the large amount of tuberculosis, chiefly gland, bone and joint affections (in youths), and the appalling consequences directly caused by drinking milk from cows affected with tuberculosis of the udder.

One can see the reason why it is so absolutely incumbent on all of us to do all in our power to assist Local Authorities in the eradication of the disease in dairy cows if we are to prevent a permanent source of tuberculosis from animal to man.

May I venture to say that the present-day trend of research seems to me quite wrong? For my own part I am frankly disappointed with the results of laboratory

investigation. They have not yielded to the practitioner the aid in the successful treatment of disease which he so urgently requires. To mention only a few cases, I ask you what is being done to probe the mysteries of some of the common diseases such as "mastitis" or to solve the problem of the various types of enteritis in cattle practice, or even "stomach staggers." These are only a few every-day problems which, in my opinion, are deserving of the immediate attention of the research worker.

My criticism of modern research is that it has become too academic. The gulf between the laboratory worker and the general practitioner is too wide and probably wider than ever it was. There is, I fear, only a slender link between them—a link which I feel in too many instances has been severed altogether. I am really pleading for a better understanding between the laboratory and the consulting room.

Research workers are born, not made, and some method of selection of these individuals should be adopted.

In conclusion, I should like to say a few words on the subjects of colleges and research institutes. We are all aware of the present state financially of our teaching institutes, a state which can be described only as deplorable; yet, within the last few years, we have had instances of two institutions being built for research. How is it that money is obtainable for these buildings when none is forthcoming for our schools? The latter we know are far from being fully equipped for teaching purposes either as regards staff or apparatus. How then can we hope to staff our research institutes with fully-trained individuals?

I submit to you that the time is ripe for a thorough revision of the state of our schools, and that that revision should take place before any further steps are taken to extend the activities of research.

It is wholly a question of finance, and some earnest endeavour should be made to impress on the Exchequer the necessity of putting our colleges in a sound financial position. We cannot expect badly-equipped schools working under conditions of direct poverty to produce men to do our profession credit either in practice or in the laboratory.

A vote of thanks to the President for his address and for his conduct in the chair was very heartily given.

J. McALLAN, *Hon. Secretary.*

"DICK" COLLEGE CENTENARY POST-GRADUATE FELLOW.

Mr. Geoffrey Bernard Brook, M.R.C.V.S., of Kinlochleven, and formerly of Sheffield, has been elected for a second period Centenary Post-graduate Fellow at the Royal (Dick) Veterinary College. Mr. Brook acted as Clinical Assistant in the College for an academic year, and was first elected to the Centenary Fellowship at the beginning of last Session. During his tenure he has conducted an investigation into the application of X-rays to veterinary surgery, and, in terms of the Fellowship, has spent a study-period in the veterinary school at Stockholm.

NOTES AND NEWS.

The Editor will be glad to receive items of professional interest for inclusion in these columns.

Diary of Events.

Oct. 16th—Meeting of the Derbyshire Division, at Royal Hotel, Derby.
Oct. 19th—Meeting of the Mid-West and South Wales Division, at Cardiff.
Oct. 20th—Meeting of the Scottish Branch, N.V.M.A., at Perth.
Oct. 22nd—Meeting of the Editorial Committee N.V.M.A., 10 Gray's Inn Square, W.C.1, at 4-30 p.m.
Nov. 21st—Annual Meeting of the Scottish Metropolitan Division N.V.M.A., at Edinburgh.

TUBERCULIN TEST ENQUIRY DEMAND IN SCOTLAND.

"At the September meeting of the Central Executive Committee of the National Farmers' Union of Scotland," says *The Farmer and Stockbreeder*, "Mr. John Speir declared that there was very widespread dissatisfaction regarding the tests administered for the purpose of ascertaining whether tuberculosis was present in dairy cattle. A recent case in Lanarkshire, where a very large percentage of a fine dairy herd had been declared to be reactors, had caused grave concern throughout the whole country and in England, and it was felt that a thorough searching enquiry into the various tests and the methods of administration was urgently called for. The Ministry of Health in England had been approached and invited to institute such an enquiry. He thought the Scottish Union should pursue the question and get a definite ruling as to whether the tests now used were valid and which of these should be laid down as the official tests and how each test should be administered. He moved that the Union should ask the Scottish Board of Health and the Board of Agriculture for Scotland to confer with representatives of the Union with a view to instituting some suitable form of enquiry into the whole matter.

"This was strongly supported by Mr. E. J. More, Airdrie, and Mr. John Muir, Dumfries, both of whom stressed the point that the tuberculin used should be standardised and the distribution of it should be under proper control."

THE POSITION OF THE VETERINARY PROFESSION IN THE SAORSTAT.

"Among the Government Bills to be introduced in the Oireachtas during the coming sittings will be one to regularise the position of the veterinary profession in the Saorstat," writes the Political Correspondent of *The Cork Examiner* to his paper on 2nd October. "At present veterinary surgeons in the Free State are in the same position as that occupied by doctors before the establishment of the separate medical register by the Act of two years ago. Those who take out their degrees in the Dublin Veterinary College are placed on the English register, but it is the desire of the Government that the Free State should have a veterinary register of its own.

"Still, however, the majority of Irish veterinary graduates practice outside the Free State. It is important that their right to have their names entered on the British register should be preserved. The matter has been the subject of several conferences between representatives of the two Governments and of the respective veterinary authorities, and agreement has so far been reached that it has been possible to draft a Bill for early introduction. It is understood that in its more important features the Bill will correspond to that governing the registration of doctors; that is to say, there will be separate veterinary registers for the two countries, with the right of persons entered in one to be registered also in the other, on payment of an additional registration fee.

Personal.

Appointments.—Mr. R. S. Marshall, M.R.C.V.S., D.V.S.M., has received an appointment to the Colony of Nigeria as Assistant Veterinary Pathologist, and Major H. Greenfield, O.B.E., M.R.C.V.S., as Veterinary Officer, Barbados.

No, not "Last of All."

"A doctor was severely criticised by a coroner and jury for not attending an injured child until three hours after the accident," says the *Daily Express*. "His explanation was in part that the parents had not paid his bills for years. The jury seemed to think that this was no explanation at all.

"But doctors have to live. They begin to earn money from their profession later than almost any other men in any other walk of life, and after an exceedingly expensive training. They are splendidly responsive to the humanitarian calls made upon them.

"Yet they may claim they should no more be expected to attend people for nothing than should a dairyman to give away his milk or a cobbler to supply free shoes on demand. The jury omitted to consider one point. They should have directed a rider against the thousands upon thousands of people who habitually leave their doctors' and dentists' bills to be settled last of all."

Veterinary Research in New South Wales.

At the last three annual sessions of the Pastures Protection Boards of New South Wales, considerable attention has been paid to the proposal to extend the policy of veterinary research now carried out at the Glenfield Veterinary Research Station, and the training of graduates in veterinary stience at the Veterinary School. University, and it is pleasing to note that of the 63 Boards in the State, 55 have agreed to contribute £2,688 of the £3,000 aimed for annually.

Predominance of Livestock in Scottish Agriculture.

The Agricultural Output of Scotland, 1925 being a report on certain statistical inquiries made in connection with the census of production, relating to the output of agricultural produce, with a survey of the agricultural statistics of Scotland from 1871 to 1925, has been issued as a Blue Book (Cmd. 3191, H.M. Stationery Office, 2s. net).

In presenting the report, the superintendent of statistics and intelligence states that the census, so far as it goes, tends to show that the output of staple products is well maintained, and in particular it exhibits the overwhelming importance of animal husbandry.

That part of the report which deals with the value of the agricultural output in 1925 shows the great predominance of livestock in Scottish agriculture. The total value of the produce sold off farms and other holdings in that year was estimated at £48,660,000. The actual sale of livestock accounted for £26,430,000, or nearly 54 per cent. of the value of the whole output. Beef alone, which was valued at £16,300,000, constituted 33.5 per cent. Milk and dairy produce brought in £8,900,000, or 18.1 per cent.; poultry and eggs £2,230,000, or 4.6 per cent., and wool £1,100,000, or 2.3 per cent., the total amount realised for livestock and its products being £38,600,000, or nearly four-fifths of the total. The corresponding proportion in England and Wales was 68.6 per cent. While, however, in Scotland the value of livestock in itself is nearly three times the value of milk and dairy produce, in England and Wales the latter is in the proportion to the former of five to seven Poultry and eggs also constitute a larger proportion of the output in England and Wales than in Scotland, while wool constitutes a smaller proportion.

The total number of horses in 1925 had changed very little as compared with 1871-75, but breeding had fallen off heavily since the War. Dairy cattle had increased by 50,000, while "other cattle," after increasing up to 1905 had in the last 20 years fallen back to about the same number as in 1871-75. Neither class had kept pace with the growth of the human population. Sheep reached their maximum in 1896-1900 and their minimum in 1920; they had since largely recovered the loss incurred in the years 1917-20. The total amount of meat produced in 1925 was estimated at 4,665,000 cwt., of which two-thirds were beef and one-fourth mutton and lamb. The total quantity of milk produced, excluding that fed to calves, was 171,000,000 gallons, or about 430 gallons per cow.—*The Times.*

Professor G. Grey Turner's N.V.M.A. Banquet Speech.

Professor G. Grey Turner, of Newcastle, writes to us to point out that our report of his reply to the toast of the University of Durham on the occasion of the N.V.M.A. Banquet at Newcastle, which was published on pages 801 and 802 of our Congress Number, contained a number of serious errors.

We sincerely regret, and unreservedly apologise for, the annoyance caused to Professor Grey Turner by the publication of an inaccurate report, for which, however, no member of the Association's staff was responsible. It appears that advantage was not taken of the Professor's offer to revise the reporter's notes before the former left for the Continent. Upon his return he found a proof from the office awaiting him, but it was then, most unfortunately too late to effect revision before publication. Professor Turner entirely disclaims the greater part of the speech as reported.

Artificial Lighting for Poultry Houses.

The National Institute of Poultry Husbandry has issued a report of an experiment conducted during the winter season of 1927-28 with a view to ascertaining the effect of lighting a house for the purpose of supplying extra food to a flock of late-hatched White Leghorn pullets. Two flocks, each consisting of 120 pullets, hatched on May 10th, were accommodated in adjoining pens, one flock being fed and managed normally, while the other had their house lighted early in the morning to enable them to begin to feed about 4 a.m. The conclusions arrived at were:—

(1) Under the conditions of the test the late-hatched pullets proved profitable, though not so valuable as earlier birds; (2) the lighted pullets gave better production and profit than the others; (3) satisfactory inexpensive devices may be arranged for automatic regulation of the lights; (4) there was no ill-health or excessive mortality resulting from artificial illumination and stimulation; (5) body-weight increased markedly among the birds under lights; (6) food consumption was necessarily materially increased as a result of lighting; and (7) the cost of lighting worked out at 2d. a bird, and the margin of income over combined

feed and lighting costs was 14s. 6.4d. a bird in the lighted flock, being 1s. 8.4d. a bird more for the 48-week period than the margin of income over feed cost in the unlighted flock.

H.M. Stationery Office Publications.

The undermentioned have been published recently. Copies can be purchased through any bookseller, or directly from H.M. Stationery Office, at the following addresses :—London : Adastral House, Kingsway, W.C.2., and 28 Abingdon Street, S.W.1 ; Cardiff : 1 St. Andrew's Crescent ; Manchester : York Street ; Edinburgh : 120 George Street.

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AGRICULTURE, MINISTRY OF.		
Anthrax Order, 1928. Diseases of Animals Branch Circular Letter No. 121 to all Local Authorities for the purposes of the Diseases of Animals Acts in Great Britain. (24-9999-00-00)	0 1	0 1½
Journal, October, 1928 (24-0099-10-28)	0 6	—
AGRICULTURE, BOARD OF, FOR SCOTLAND :—		
Agricultural Statistics, 1927, Vol. XVI. Part I. Report on the acreage and production of crops and number of live stock in Scotland. (47-0007-01-27)	3 0	3 1
Vol. XVI. Part 2. Prices of Grain, Livestock, and other agricultural produce in Scotland and supplies of Livestock at certain markets. (49-0007-02-27)	1 3	1 4
STATUTORY RULES AND ORDERS.		
626/S.40. Dogs, Scotland. Order, dated August 4th, 1928	0 1	0 1½
634. Dangerous Drugs, Order in Council, fixing September 25th, 1928, as the appointed day for the commencement of the Dangerous Drugs Act, 1925, dated August 14th, 1928	0 1	0 1½
644. Dangerous Drugs, Order in Council, applying Part III of the Dangerous Drugs Act, 1920, to certain other Drugs, dated August 14th 1928	0 1	0 1½

Home Secretary and Stray Dogs.

The Home Secretary has given intructions in regard to the accommodation for stray dogs at police stations, in accordance with his recent promise in reply to a question asked in the House of Commons by Sir Robert Gower, the Chairman of the National Canine Defence League. It is understood that new kennels are being installed in certain police stations, while in others the accommodation is to be brought up to a higher standard. The arrangements made for stray dogs at each station will henceforward be taken into account by H.M. Inspectors of Constabulary when making their inspections necessary to the granting of a certificate of efficiency, upon the issue of which depends the Treasury grant toward police maintenance.

Paris Horses Doubled since Motor Advent.

"Although the British visitor to Paris who tries to cross the Place de la Concorde at midday may hardly credit it, the number of horse-drawn vehicles in Paris has doubled in the last twenty-five years, despite the motor-car," says a British United Press message to *The Evening News*.

"Horse-drawn vehicles in Paris to-day number 30,000 and the increase in horse-drawn cabs has kept pace with the increase in the population. Only the stately hansom has disappeared from the streets."

We hear that Mrs. Kirk made a splendid effort in Golder's Green last week in aid of National Dog Week. She organised a flag day, the proceeds of which totalled £25. We have, so far, not heard of any veterinary surgeon's wife beating this record, and think she is to be highly complimented.

CORRESPONDENCE.

Letters to the Editor should reach the Office not later than by the first post on Tuesday morning for insertion in following Saturday's issue.

All correspondence must bear the name and address of the contributor for publication.

The Editor does not hold himself responsible for the opinions of his correspondents

Congress Crucial Incision Operation.

TO THE EDITOR OF THE VETERINARY RECORD.

Sir,—At 9-30 on the morning of the operation day at Newcastle, the surgeons were asked to meet the professional stewards at the Secretary's office to give instructions. The only request I made with regard to the cow which was the subject of the above operation, was that on no account was any milk to be taken from the quarter to be operated upon, as this point was frequently of the utmost importance. On going up to operate I was met by the steward who reported that in spite of his efforts the quarter had been milked out considerably. After the operation, in giving instructions to the veterinary surgeon who provided the subject, I suggested that the teat should be operated upon again, if this time unsuccessful, after the next calving. The operation at the Congress consisted of three small skin incisions only, the incision "guides" (ring and handle) were not inserted into the previous incision but the instrument was only used as a director. The instrument, when used in its proper fashion, under suitable conditions, serves as a sphincter "director," incision "guide" and incision "control." Under unfavourable circumstances, such as those met with at Newcastle, one has to do one's best, taking no risks, whereas in private practice, one would go back the next day when there would be no risk and operate under safe conditions, where the instrument could be used as it is intended. This letter is written for the perusal of those intending to take up the operation, lest they should have misgivings.—Thanking you, I am, yours faithfully, OSCAR STINSON.

Appleby,
Westmorland.
October 6th, 1928.

Pneumo-enteritis in Puppies.

TO THE EDITOR OF THE VETERINARY RECORD.

Sir,—I have received a letter from Mr. J. R. Stewart, Veterinary Surgeon, Sydney, from which I am quoting below, in the expectation that his suggestions as to treatment will be of interest.

"We have a somewhat similar complaint, and where I have been called and found pups dying, I have advised clients to call me next time as soon as the pups are born. I then collect (for eight pups) one ounce of blood, in citrate solution from the mother, from the vein on the inner aspect of the carpus and immediately inject 3—4 c.c. (1 c.c. in each of four sites) subcutaneously into each pup and massage to disperse. With this treatment I have no losses. It might be worth a trial with you."

It would be interesting if veterinary surgeons adopting this treatment would record their results.—Yours truly,

R. H. SMYTHE.

Basset Road,
Camborne,
October 2nd, 1928.

The Editor acknowledges the receipt of the following :—
Communications from Messrs. A. D. McEwan and W. Litt.

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